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Conquest, Capitulation, and Indian Treaties

CHARLES GIBSON

IN THE FIRST PART OF THIS ADDRESS I shall describe the results of an investigation into a particular historical problem. In the second part I shall select from that investigation and that problem some topics of more general application and interest.

THE PROBLEM BEGINS WITH A QUESTION relating to Indian treaties. The question was posed some time ago by my colleague Robert Berkhofer in the course of a conversation on the varieties of European colonization in North and South America. Did the Spaniards ever make treaties with Indians? We knew that in North America the English and the French made treaties with Indians. The Dutch did so in the Hudson Valley. The Portuguese made treaties with the natives of Brazil. But why did we seem to lack evidence for Spanish treaties with Indians? And if indeed Spaniards did not make such treaties, how and when did the tradition of Indian treaty-making come to be established in this hemisphere? The question was the more intriguing because at the University of Michigan we place a special value on historical comparison. We once conducted a program precisely on comparative colonization in the Americas, identifying and contrasting the points of difference among the colonizing nations. Mr. Berkhofer's question concerning Indian treaties thus struck a familiar chord, though it concerned a subject that had been quite overlooked in our previous investigation.

By way of response, I could recall some *ad hoc* alliances that Spaniards had made with Indians, as well as some loose oral agreements, promises of reward, and the like. Long ago I made a study of one of these supposed agreements, between Fernando Cortés and the natives of the province of Tlaxcala in Mexico, concluding that the matter had been much exaggerated, especially by the Tlaxcalan Indians themselves, who stood to benefit from the exaggeration in various ways.¹ But even if the Tlaxcalan example could

This presidential address was delivered at the annual meeting of the American Historical Association in Dallas, December 28, 1977.

¹ *Tlaxcala in the Sixteenth Century*, Yale Historical Publications, Miscellany, no. 56 (New Haven, 1952), 159–61. Another well-known example occurs in the conquest of Peru, where Pizarro notified the followers of Atahualpa that he would support them against the followers of Huascar, and where he notified the

qualify as an agreement between Spaniards and Indians, one could still not speak of a treaty—supposing that a treaty is more formal and official than a simple agreement and that it is written and signed. Indeed, in the whole early history of Spanish America I could not recall anything that could properly be identified as an Indian treaty. It did seem to be the case that treaty-making had played more of a role in the policies of the other colonizing nations than it had in the policies of the Spanish.

A conclusion so tentative would surely prove inadequate for the series of related questions that were bound to follow. But it was at least congruent with the known character of the Spanish conquests in America. The conquistadores overran native American civilizations with such speed and vigor that few opportunities arose even for loose oral agreements. The first years of the Spanish American colony were years of license, dominated by private greed, in the absence of governmental sanction. Spaniards understood Indian peoples to be royal subjects, ready for Christianization and exploitation, but inappropriate for the kinds of bargaining and negotiation that might have resulted in treaties. And in all or most of these respects the Spanish experiences and attitudes differed from those of the other nations.

Of the related questions, assuming that we were correct so far, the most immediate and insistent concerned cause. If the Dutch, French, Portuguese, and English did make treaties with Indians and if the Spaniards did not, what could be the reasons for this difference? Was it a circumstantial matter, dependent simply on the location of the several colonies and the nature of the Indian tribes encountered? Or might one connect it with other attributes of the Hispanic world, or of the Dutch, French, English, and Portuguese worlds? Could some other historical tradition be shown to lie behind the different approaches to Indian treaties, possibly in European relations with non-Europeans (we thought vaguely of Africa) or in Christian relations with pagans at some earlier time? If so, what were the dimensions of that tradition, and why did Spaniards inherit, if they did, a legacy different from the others? Clearly this was a subject for which one needed more information on the European, and especially the Iberian, antecedents of American colonization.

In the Iberian peninsula the overriding institutional prototype and parallel to the Spanish conquests in America was the eight-hundred-year *Reconquista*, the Christian recovery of Spanish territory from the Moors, beginning in the year 718 in the Pyrenees in the north and ending in 1492 with the capture of Granada in the south. It is a widely held tenet of Hispanic studies that the reconquest heritage in Spain bears some relation to the history of conquest in the New World, as if the energies of the campaign against Granada spilled out into the overseas world at the same time that America was being discovered.

followers of Huascar that he would support *them* against the forces of Atahualpa. One could call such agreements treaties perhaps, but I think that most historians would prefer not to do so. Philip W. Powell, *Soldiers, Indians, & Silver: The Northward Advance of New Spain, 1550–1600* (Berkeley and Los Angeles, 1952), 187, does speak of “peace treaties” on the northern frontier of New Spain in the period of Viceroy Villamanrique. But it does not appear that these were written, signed agreements in the tradition of European treaty-making. The question still remains problematical.

The two events, reconquest and conquest, were alike in that each was an expansionist war, each involved Christian penetration into non-Christian territories, each was declared by the papacy to be a crusade,² and in each the non-Christian captives were employed or sold as slaves. Scholars as knowledgeable as Claudio Sánchez Albornoz have argued forcefully that the peninsular *Reconquista* and the American conquests were closely related phenomena.³

Reading further on these subjects I became aware of the many treaties and treaty-like documents drawn up between Christians and Moors during the *Reconquista*. Whatever other similarities there might have been between peninsular reconquest and American conquest, it appeared that in this matter of written agreements there was a major difference. Spanish reconquest history offered many examples of written agreements. Spanish American conquest history seemed to offer none. If it could in fact be established that Spaniards—unlike the other colonizers—refrained from making treaties with Indians, this could hardly have been a policy arising out of the *Reconquista*. *Reconquista* history suggests that the reverse should have been the case. Seemingly Christian Spaniards abandoned treaty-making in their dealings with non-Christians, and—whether by coincidence alone or for some reason not yet perceived—this presumed abandonment coincided in time with the discovery of the New World. Seemingly also the other nations, of course with much more limited traditions of Christian versus non-Christian warfare, adopted treaty-making some time after the Spaniards gave it up.

Among the types of agreement in the Spanish *Reconquista* the *tregua*, a peace accord or truce, and the *capitulación* (capitulation) stand out. Peace accords were arranged between the Christian and the Moslem states as sovereign entities at war with each other. Thus in the fifteenth century—the final hundred years of the conflict—a treaty of peace signed in 1410 between Christian Castile and Moslem Granada was renewed for a time, then broken, and then replaced by peace treaties of 1439, 1464, and other years.⁴ Castile and Granada were separate states at this time, and the treaties mark the intermissions, the periods of recuperation, in their prolonged hostility. Capitulations, which are more complicated and, in the present context, more relevant, appeared when Moslems submitted to Christians, principally (at least as far as my reading went) in the final decade of the Granada war, between 1482 and 1492. Capitulations might be signed by the Christian monarchs alone or by the Christian monarchs and the local Moslem leader together, and they recorded the privileges granted or the conditions imposed by the Christian

² Antonio Antelo Iglesias, "El ideal de cruzada en la baja edad media peninsular," *Cuadernos de historia: Anexos a la revista Hispania*, 1 (1967): 37–43; and Berthold Beinert, "La idea de cruzada y los intereses de los príncipes cristianos en el siglo XV," *ibid.*, 45–59. On indulgence as the criterion for crusade, see José Goñi Gaztambide, *Historia de la bula de cruzada en España* (Vitoria, 1958), 46, *passim*.

³ Claudio Sánchez Albornoz, *España, un enigma histórico*, 2 vols. (Buenos Aires, 1956), 2: 501, *passim*.

⁴ José Amador de los Ríos, "Memoria histórico-crítica sobre las treguas celebradas en 1439 entre los reyes de Castilla y de Granada," *Memorias de la Real Academia de la Historia*, 9 (1879): 1–153; Juan Torres Fontes, "Las treguas con Granada de 1462 y 1463," *Hispania*, 23 (1963): 163–99; and Miguel Angel Ladero Quesada, *Granada, historia de un país islámico (1232–1571)* (Madrid, 1969), 105–18.

conquerors on their newly incorporated subjects. Thus one could argue that they were not exactly treaties. But in all the cases that we know the capitulations were as much the outcome of negotiations between the two parties as were the peace treaties themselves, and they were very close to treaties in their historical significance and role.⁵

It will come as a surprise to some, as it did once to me, to learn that the term capitulation did not refer to surrender. What we have in a *Reconquista* capitulation is literally a series of chapters—*capítulos* in Spanish—or stated terms or conditions or subheads of an agreement. Indeed *capitulum* means subhead, for it is a diminutive form of *caput*, head, and the subheads provide the specification or enumeration of the items agreed upon. When we view the matter in this light, a number of other historical puzzles fall into place. Capitulations were lists of conditions that Holy Roman Emperors were expected to obey, and it is from this same perspective that we can understand why the famous agreement between Ferdinand and Isabella, on the one hand, and Christopher Columbus, on the other, signed at Santa Fe a few miles from Granada in April 1492, is entitled “Capitulations of Santa Fe.”⁶ The term capitulation, in both its Spanish and its English form, came to mean surrender only in a subsequent usage, presumably because acts of surrender, perhaps especially in the seventeenth century, were so often followed by capitulations, or enumerations of terms agreed upon. With this a connection was established—limited and verbal to be sure—between surrender, which is what Indians did to Spaniards in America, and agreement or treaty, which is what we were inquiring about in the first place. The one English vestige of the earlier sense that I think of is *recapitulation*—the “recap” of the media anchormen—which is a summing up of the principal points or subheads of what has already been said.

For the Granada war of the late fifteenth century, the historian has at his disposal an abundantly documented capitulation history. The actual capitulation terms, or contemporary or nearly contemporary accounts of the capitulation terms, survive for a number of Moslem towns taken by Christians in these years.⁷ When one deals with historical evidence of this type, a useful technique, in my experience, is to tabulate the critical features, and I am now in the process of making tables of the names of the towns captured in the Granada war and the capitulation conditions for each. I have decided to spare you the details. But let us look for a moment at what we may call a standard Granada capitulation, to identify the type and above all to see the matter from a point of view that will be meaningful in relation to subsequent Spanish dealings with Indians in America.

⁵ Miguel Garrido Atienza, *Las capitulaciones para la entrega de Granada* (Granada, 1910). The term was most often used in reference to agreements between explorers and the monarchy. But *capitulación* could also be used in reference to a genuine treaty, e.g., the Treaty of Alcaçovas (1479) or the two Treaties of Tordesillas (1494). José López de Toro, ed., *Tratados internacionales de los Reyes Católicos con algunos textos complementarios*, 2 vols. (*Documentos inéditos para la historia de España*, 7, 8; Madrid, 1952), 1: 125–78; 2: 22–40, 41–58.

⁶ Germán Bleiberg, *Diccionario de historia de España*, 3 vols. (Madrid, 1968), 1: 672–74.

⁷ Examples are Almería, in *Colección de documentos inéditos para la historia de España* (hereafter *CDIHE*), ed. Martín Fernández Navarrete *et al.*, 112 vols. (Madrid, 1842–95), 11: 475–79, and Purchena, in *ibid.*, 7: 403–07. Additional texts of this kind were assembled by Garrido Atienza, *Las capitulaciones para la entrega de Granada*.

The capitulations of the 1480s and early 1490s identified the newly incorporated Moslems of Granada as *vasallos*, vassals of Ferdinand and Isabella. No longer enemies, they were to give up their arms. As vassals they were to receive the protection of the crown and live under their own laws with their traditional customs and religion. They would henceforth pay tribute to Ferdinand and Isabella, but the amounts to be paid would be no greater than those previously paid to their own Moslem emir. They could remain where they were, with their property; they could move north into Castile and take up a new life there; or they could leave Spain and go south to Africa. If they chose emigration, their passage would be paid by the Christian state and they would sail in ships that Ferdinand and Isabella themselves would provide. As later in America two societies, two *repúblicas*, were projected for Granada, one dominant, the other subordinate. But from the vantage point of the student of colonial Spanish America the remarkable feature in Granada in the 1480s and 1490s is the promised preservation of Mohammedanism and of the non-Christian way of life in the subordinate society. In Mexico and Peru, thirty, forty, and fifty years later, the Aztec and Inca religions were to be destroyed. But in conquered Granada in the late fifteenth century Mohammedanism was to be respected, Islamic law would prevail, the rents of the Granada mosques would be protected by the Catholic monarchs, and Christians would be forbidden even to enter the Moslem places of worship.⁸

Individual Granada capitulations varied somewhat from this standard form, depending upon local conditions and the progress of the war. The city of Granada itself, with the fall of the Alhambra and the conclusion of the whole campaign in 1492, received the most liberal terms of all.⁹ But the case to which I would particularly invite your attention, because it stands as an exception to the continuing capitulation principle here, is that of the Moslem city of Málaga, taken by the Spanish Christians in the summer of 1487. Twice during the siege of Málaga the Christian attackers offered capitulations with standard terms. On each occasion the offer was rejected by the determined Moslem garrison. Finally the exhausted civilian inhabitants of Málaga found themselves ready to yield, and they persuaded the garrison to stand aside and let them, the populace, under the leadership of a local merchant, negotiate with the Christians. Overtures were made to this end. But it was the Christians who now proved adamant. Málaga's request to agree on the terms of peace was rejected by Ferdinand on the grounds that the city had twice been offered the opportunity and had twice refused and now it was too late. Instead of achieving peace under the liberal capitulation terms of other Granada towns, Málaga was then conquered and subjugated, and the majority of its inhabitants were held to ransom as slaves. Málaga thus offers us a deviant case, one that resembles, in its severity and in the absence of written agree-

⁸ Many historians have dealt with these capitulations. A well-constructed summary treatment is Miguel Angel Ladero Quesada, *Castilla y la conquista del reino de Granada* (Valladolid, 1967), 69–97. On the meaning of *vasallo*, consult Alfonso María Guilarte, *El regimen señorial en el siglo XVI* (Madrid, 1962), 158 ff.

⁹ *CDIHE*, 8:411–36.

ments, the Spanish enslavement practices in the West Indies much more than the customary leniency of the Granada war. But why should the exception in Spain before 1492 have become the rule in America after 1492?¹⁰

In searching for our answer to this question, we should not proceed immediately to the West Indies or Mexico or Peru. As we contemplate the transition from reconquest to conquest, we become aware that as with so many other transitions in historical reconstruction some intermediate steps must be taken. Spanish expansion did move from the peninsular reconquest to the American conquests. But en route we find diversions into other areas, to the point at which the basic transition appears much less immediate than it did at first. North Africa—close at hand, Moslem, and with a long heritage of contact with Spain—offered the most natural continuity. Within months after the fall of Granada, Ferdinand and Isabella were sending spies to reconnoiter the cities of North Africa. Two decades later Spanish forces had occupied most of the prominent Moslem strongholds along the Mediterranean coast of Africa for a distance of over one thousand miles.¹¹ It seems an extraordinary expansion, when we consider that the whole *Reconquista* distance from the Pyrenees to Granada is only four hundred miles, and it thus prefigures the accelerated pace of Spanish expansion in North and South America.

The point of interest for our present inquiry is that capitulations *were* drawn up between Christians and Moslems in North Africa. They contained provisions very similar to those of the recently completed Granada campaign. As in Granada the North African capitulations specify peace, vassalage to the Spanish crown, preservation of existing Moslem laws and customs, maintenance of Mohammedanism, and payment to Spain of the tribute previously paid to the Moslem rulers.¹² Nothing is said of religious conversion or forced labor or other topics familiar in colonial Spanish America. Thus the evidence is unmistakable that Spaniards carried the *Reconquista* capitulation tradition intact or almost intact to areas outside the Iberian peninsula, into another continent, and into the sixteenth century. We have come to a time when Spaniards were already involved in the West Indies, we have come close to the historic conditions of America itself, and the capitulation principle remains strong. But how much farther can it continue?

We look then to a second transitional area, the Canary Islands, in the

¹⁰ The siege of Málaga is described in a number of chronicles of the period, e.g., Andrés Bernaldez, *Memorias del reinado de los Reyes Católicos*, ed. Manuel Gómez-Moreno and Juan de Mata Carriazo (Madrid, 1962), 180–94. W. H. Prescott gives a graphic account: *History of the Reign of Ferdinand and Isabella the Catholic*, ed. John Foster Kirk, 3 vols. (Philadelphia, 1872), 2:17–42. Scholarly modern treatments are Miguel Angel Ladero Quesada, *Castilla y la conquista*, 72–77, and “La esclavitud por guerra a fines del siglo XV: el caso de Málaga,” *Hispania*, 27 (1967): 63–88. The capitulation for ransom is published in *CDIHE*, 8: 399–402. Of course I do not mean to equate the Málaga campaign and the American conquests in every respect.

¹¹ The African campaigns are treated in some detail in the chronicle of Lorenzo de Padilla, *CDIHE*, 8: 5–267, and Gerónimo Zurita (Zorita) y Castro, *Anales de la corona de Aragón*, 7 vols. (Zaragoza, 1510–21), 6: 211 ff. See also Angel Canellas, *Fuentes de Zorita: Documentos de la alacena del cronista relativos a los años 1508–1511* (Zaragoza, 1969), 39 ff., and the relevant documents. A pioneering modern study is Fernand Braudel, “Les espagnols et l’Afrique du nord de 1492 à 1577,” *Revue africaine*, 69 (1928): 184–233, 351–418. More recent scholarship is summarized in the volumes of *Curso de conferencias sobre la política africana de los Reyes Católicos*, 6 vols. (Madrid, 1951–53). Many other sources might be cited.

¹² The best documented instance is Algiers in 1510. See Zurita y Castro, *Anales*, 6:211.

Atlantic off the African shore. It is another Spanish conquest zone, and it shares the traits that we have identified above for reconquest and conquest: expansionist war, strife between Christian and non-Christian, official classification as crusade,¹³ and enslavement of captives. The Canary archipelago, intermediate between Europe and America, has much to tell us about both sides of the Atlantic, and I might add that Canary Island historiography, like peninsular Spanish historiography, has been wonderfully improved and modernized in recent decades.¹⁴ But we want to concentrate for the moment on the question of treaties. Did the Spaniards make written agreements with the natives of the Canary Islands?

They did, though the evidence for their doing so still leaves something to be desired. Texts of the late fifteenth century make reference to peace treaties and capitulations, as well as to other agreements generally referred to as *pactos*, drawn up between Spaniards and Canary Islanders. The natives were pagans like the Indians of America, and not infidel Mohammedans as were the inhabitants of North Africa, and the *pactos* promised freedom from enslavement to those who converted or who promised to convert to Christianity. Unquestionably the military conquest of the Canaries was attended by a stronger Christian conversion program than were the conquests of Moslem Spain and the North African cities. By ancient tradition in Christendom, a captive taken in a supposedly just war could be legitimately enslaved, as many were in the peninsular *Reconquista*, in North Africa, in the Canary Islands, in America, and of course in the best known slave area, likewise developing at this time, Portuguese Angola. But especially in the Canary Islands an individual or a tribe or the inhabitants of a particular area might be exempted from such enslavement in advance through *pactos*.¹⁵

The Canary *pactos* thus enlarge and enrich our repertory of agreements between Christian and non-Christian. Rather than deteriorating as our investigation moves farther out into the Atlantic, the written-agreement tradition appears to be flourishing and still expanding. This is not what we would expect if it is true that Spaniards refrained from making written agreements in America, and it appreciably narrows the margin to which we may look for the turning point in policy.¹⁶ It would appear that there must be some crucial differences yet to be identified between the conditions of our “background” areas and the conditions of America.

¹³ On this subject, see Goñi Gaztambide, *Historia de la bula de cruzada*, 334 ff., and Antonio Rumeu de Armas, “Los problemas derivados del contacto de razas en los albores del renacimiento,” *Cuadernos de historia: Anexos a la revista Hispania*, 1 (1967): 79 ff.

¹⁴ Studies published in the *Revista de historia* (Tenerife) and the *Anuario de estudios atlánticos* are basic to this development. On the relation with America, see Silvio Zavala, *Estudios indios* (Mexico, 1948), 7–94.

¹⁵ The bull *Pastor bonus* (1462) of Pius II speaks of a “pactum pacis et confoederationis aut securitatis” with the natives of the Canary Islands. See Joseph M. Pou y Martí, ed., *Bullarium franciscanum, continens constitutiones epistolae diplomata Romanorum Pontificum Calixti III, Pii II, et Pauli II ad tres ordines S. P. N. Francisci Spectantia*, n.s., 2 (Claras Aquas [Quaracchi], 1939): 546. A good summary is Antonio Rumeu de Armas, *La política indigenista de Isabel la Católica* (Valladolid, 1969). Rumeu de Armas’s Document 31 is the ratification by Ferdinand and Isabella of a *pacto* with natives of Gran Canaria. But I have yet to see an actual *pacto* text.

¹⁶ A full treatment of this subject would consider also the Spanish conquests and colonization on the Atlantic coast of Africa, which was far from being exclusively Portuguese. Antonio Rumeu de Armas, *España en el Africa atlántica*, 2 vols. (Madrid, 1956–67), discusses this subject.

We still cannot be certain what these crucial differences were. Did the great wealth of Aztec and Inca civilizations induce the change? Was it that Christian Spaniards retained a respect for Mohammedanism that they were unable to transfer to the native American religions? We need to know much more than we do know about the Jiménez de Cisneros reforms and, rather surprisingly, the real reasons behind the Spanish Christianization of the American Indian. In addition we may have made a basic error in assuming that the American conquests followed so closely the traditions of the Spanish *Reconquista* and the campaigns in North Africa and the Canary Islands. Some perceptive scholars in recent years have sought the origins of the American conquests in another kind of activity: the *cabalgadas*, private raids, and forays by sea, made by Spaniards against African natives and Portuguese, of course in the absence of capitulations or treaties with the enemy.¹⁷ Perhaps we should look much more to the very early Spanish experiences in the West Indies for the precedent-setting decisions that prohibited treaties with Indians. Then again it must be remembered that the lenient Christian capitulations with the Moslems of Granada had themselves been abrogated by the time of the conquests of Mexico and Peru. Thus we have some leads for further research. But it remains true that our inquiry, having brought us to the brink of the Spanish colony in America, has still failed to explain the critical differences between that brink and the colony itself.

IT DOES NOT REALLY BOTHER ME that we may fail. All inquiries fail in some sense, and in scholarship as in other pursuits one is prudent to be content with a limited outcome. Moreover, a presidential address sets its sights on more than the simple recounting of a single investigation, whether success or failure, and we must move on. Our association's presidential addresses provide occasions for summaries and conclusions and lessons. They connect the problem under consideration with other historical problems and with a theory of problems. They make the subject being examined relevant to all history. They notify the young historian where we have been and where he is and what topics and methods he may usefully make his own. I cannot do all that. But I am tempted to offer a few further remarks, and in what follows my effort will be to place the topics we have just considered in some larger perspective.

A first point concerns failure and the reporting of failure. I think that we too often represent our researches as successes. Insistence on success can distort what our researches reveal by concealing some of their most important implications. In experimental science we have recently read of outright fraud committed by persons whose careers and reputations depend on the appearance of successful findings. That so noble an occupation as the search for truth can, in the conditions of our society, engender deception of this kind is

¹⁷ Richard Konetzke, *El imperio español: Orígenes y fundamentos*, trans. Felipe González Vicén (Madrid, 1946), 22 ff., 33, *passim*; and Mario Góngora, *Studies in the Colonial History of Spanish America*, trans. Richard Southern, Cambridge Latin American Studies, no. 20 (Cambridge, 1975), 1 ff.

an appalling irony. But will anyone claim that historical scholarship is free from similar dangers? The problem is a general one. It is related to the rapid inflation of knowledge, to the multiplication of specialties, to too pragmatic an understanding of what constitutes a contribution to knowledge, and to the insidious doctrine of “publish or perish.” The hunch that fails, the dubious assumption that goes conveniently unexpressed, the hypothesis that does not fully work out, the confusion that remains unclarified—as our graduate students discover to their dismay, these are more common in historical writing than would at first appear. I would argue that admitting them would not simply be in accordance with the doctrine that honesty is the best policy. I would argue that the admission of our occasional or frequent failures would improve our discipline and make us better historians.

A second point concerns the connection between historical inquiry and the secondary literature. We fail also as historians so long as we simply accept the words and ideas we read. I am thinking of the Capitulations of Santa Fe, the contract between Columbus and the Spanish monarchs in 1492. We thought we understood this title when we learned that the celebrated documents—for there were really several—were signed and agreed to in the siege city of Santa Fe. But why were royal contracts with Columbus and other explorers called capitulations? I had sometimes wondered if Columbus was supposed to have capitulated (surrendered) to Ferdinand and Isabella, or Ferdinand and Isabella to Columbus. Vaguely I had thought that when the crown made a capitulation with Columbus, or with any explorer or indeed with anyone, it was temporarily surrendering—“yielding” and “giving up” carry the same double meaning as “surrendering” here—some portion of its sovereignty. But this was incorrect, and, when we understand the meaning of capitulation, it makes no sense.¹⁸ So far as my knowledge went, no one dealing with Columbus, not Samuel Eliot Morison or anyone else, ever explained this, or even recognized it as something to be explained.¹⁹ W. H. Prescott used the phrase “treaty of capitulation,” which I take to be a clear indication of misunderstanding.²⁰ One can see in the tone and wording of other writings in English and Spanish and additional languages too that sixteenth-century capitulation is something that is generally misunderstood in the twentieth century. I have read in twentieth-century writing that the articles of marriage between Ferdinand and Isabella were aptly named a capitulation of matrimony, the point being that Ferdinand’s role was slated to be secondary to the queen’s. But this is far off the track. Words, too, have their history. I paraphrase Marc Bloch in saying that, when we practice the art of history, we slip into presentism and anachronism more readily than we realize.

An additional and related matter concerns specializations in history and the

¹⁸ In the best known case of Indian removal in Spanish America, the transfer of Tlaxcalans to the north to serve as teachers for the uncivilized Indians of the frontier in the late sixteenth century, the official document is called *Capitulaciones*. See *Colección de documentos para la historia de San Luis Potosí*, ed. Primo Feliciano Velázquez, 4 vols. (San Luis Potosí, 1897–99), 1: 177–83.

¹⁹ I could easily have missed it. The literature on this subject is enormous.

²⁰ Prescott, *Reign of Ferdinand and Isabella*, 2: 452.

boundaries that separate one specialization from another. At least for the Hispanic world I think it would be accurate to say that historians have kept the European side and the American side too far apart. The separation of American history from European history results from our self-conscious professionalism and our continued efforts to probe further into the same areas where we began. The implications carry over into our bibliographies, our researches, the courses that we take as students, and inevitably the courses that we offer as professors. In departments of Spanish, and even in departments of history, we may find factional divisions between Europeanists and Americanists. "Europe" and "America" are useful categories, to be sure, and we depend on them in much that we properly do. But in our dependence on the one or the other we neglect their interrelationship.²¹ I would add that we do the same with many other familiar historical categories, including chronological ones such as medieval and early modern. In Spanish American studies a huge gulf has traditionally separated researches on the pre-Columbian civilizations from researches on the colonial period, to the untold detriment of both. Specialization has moved us forward on many fronts. But I am saying that we institutionalize specialization beyond what is required and that our studies suffer accordingly.

This brings us to still another topic, or pair of topics, motivation and explanation, and the relation between them. Why was it that the Christian mission program assumed a larger dimension in the Canary Islands than in the *Reconquista* and North Africa, as if preparatory to the still larger dimension it would achieve in Spanish America? In our explanations of this should we emphasize, or even mention, the possible materialistic answers: that conversion was a step in the process of claiming land, that it helped to provide a title for territories lying outside the reconquest zone, that it reconciled the papacy and turned an expansionist conquest into a "just war"? Or could we draw this question still farther into the domain of our old standby in historical interpretation, the economic motive, noting that through bulls of indulgence, especially in the 1460s and 1470s, money became available for Christian conversion in Guinea and the Canaries? It is true that Ferdinand and Isabella were attracted to these funds and that they used them to bear the costs of warfare in the Canary Islands. A change from funded Christianization to funded war for Christianization did occur at this time, and religious war persisted in the history of the Canary Islands through the period of our attention.²²

I want to express skepticism concerning such supposed motives and sup-

²¹ See on this subject the presidential address of Carlton J. H. Hayes, "The American Frontier—Frontier of What?" *AHR*, 51 (1945-46): 199-216. On the combined European and American themes, see J. H. Elliott, *The Old World and the New, 1492-1650* (Cambridge, 1970); and Fredi Chiappelli, ed., *First Images of America: The Impact of the New World on the Old*, 2 vols. (Berkeley and Los Angeles, 1976).

²² Rumeu de Armas, *Política indigenista*, 39 ff. The text of *Pastoris aeterni* (1472) and the royal cedula authorizing a military application of indulgence funds (1479) are published in *ibid.*, 151-57, 188-89. Note also the use of papal funds by Enrique IV; see Tarsicio de Azcona, *Isabel la Católica: Estudio crítico de su vida y su reinado* (Madrid, 1964), 58 ff.

posed explanations, both because they are too materialistic and because they are too simplistic. There surely are reasons for the enlargement of the Spanish missionary program in the fifteenth and sixteenth centuries, as there are reasons for any historic change, but I doubt that we shall find them by looking at the royal revenue of Ferdinand and Isabella or their successors. Ferdinand was Machiavelli's favorite king.²³ But we should be certain of what we are doing before we attribute "Machiavellianism" to Ferdinand or anyone else. What I find in history is that people are more likely to deceive themselves than to succeed in deceiving others—though I grant some spectacular exceptions to this rule. Is it not an important fact that we ordinarily remain uncertain of the real motivation of our own political leaders and even our own acquaintances and friends, not to mention ourselves? How to arrive at reasonable, and reasonably accurate, estimates of motivation in past time remains one of the historian's foremost responsibilities, and it is not something to be undertaken lightly. In my experience, students of history writing examinations and term papers, as well as their elders writing monographs and textbooks, characteristically underestimate the complexity of this subject, identifying motives that are too exclusive, too simple, or too hypothetical, and supposing that these provide adequate historical explanations.

Our final point concerns what is voluntary and what is involuntary in history and the attitudes of historical personages and historians toward these. We have here one of the oldest and most persistent problems of our discipline, and I propose to comment on it at a little greater length and connect it again and more explicitly with conquests, capitulations, and Indian treaties. There can be no doubt that Spaniards placed an emphasis on the presumed freedom of choice of the peoples with whom they dealt. With respect to Christian conversion, the prior volition of the convert was an obvious prerequisite. There existed a powerful tradition in Christianity denying the efficacy and forbidding the use of force in the process of conversion. To become a good Christian one should *want* to become a good Christian; one could not be compelled to become a good Christian. The doctrine of peaceful conversion did not prevail everywhere, of course; but it was an important view in the fifteenth and sixteenth centuries, and with some exceptions it was adhered to during the postconquest Christianization of Spanish America.²⁴

When a religious belief has been identified, a valuable exercise in historical reconstruction is to look for secular counterparts to it. There does exist a secular counterpart to this matter of the voluntary Christian convert, one that has not always been appreciated, at least in the histories that I have read. It is the belief that to be a good vassal of Ferdinand and Isabella one had to want to be their good vassal. Note the concern in the capitulations for the voluntary character of the submission of conquered peoples. Note the repeated Spanish disavowal of force. "By your own good act of will," Ferdinand stated in the

²³ See Ramón Menéndez Pidal, *Los Reyes Católicos según Maquiavelo y Castiglione* (Madrid, 1952).

²⁴ On the subject of peaceful conversion, and the related subjects of just war, enslavement, crusade, papal authority, and "spiritual conquest," there is a large and complex bibliography.

concluding capitulation with Algiers in April 1510, “you have been and are my vassals, and vassals of my royal crown, and you have sworn under your law that you will render faithful vassalage to me now and forever.”²⁵ Some of our Hispanic sources present the matter of volition with an insistence and redundancy and legalism that appear almost ludicrous to modern eyes. Consider the following, from the capitulation of Boabdil, king of Moslem Granada, prior to his return to Africa in 1493: “I, King Muley Boabdil, voluntarily, freely, spontaneously, in a manner acceptable to myself, make known that I wish, I consent, I am pleased, to consider this capitulation and everything in it good, firm, agreeable, strong, stable, and valid now and for all time.”²⁶ An obvious purpose of the capitulation was to place on record the promises, favors, and all the conditions to which the Spanish monarchs obligated themselves. But it would seem that another purpose was to record the willingness, or supposed willingness, of the other side to render vassalage and submission.

Would it be accurate to say that to modern eyes this insistence on the willingness of the other side seems hypocritical? Our twentieth-century inclination is to observe that overt force was applied by Spaniards in all the areas under consideration and that the non-Christian enemy would hardly have yielded to the Christians without such force. Our instinct as analytical historians is to examine the competitive features of the situation, to trace the process by which the loser yielded under duress, to note the progressive elimination of alternatives, and to view the outcome as the result of coercion. We are not “wrong.” But notice that, in taking the view that we characteristically take, we are reflecting our own society’s special awareness of influence and force. Inevitably, if a defeated people agree to become the vassals of a victorious monarchy, we attribute their doing so to the force that defeated them. They have, we say, no other choice. But in the Spanish mentality of the fifteenth and sixteenth centuries this was not the case. In religion one could reject Christianity and run the risk of enslavement—the wrong choice, perhaps, but nevertheless choice. Choice included the question of the timing of the choice. In the secular sphere one could fight on, choose not to yield, and wait too long, as did the defenders of Málaga. Clearly, the thesis of volition did not imply that force was wholly lacking. Choice might be strongly influenced, as in the Inquisitorial tortures, and, in situations like those that we are considering, success was measured by the degree to which choice could be properly induced. To the Spanish mind of the early modern period, this was not the same as forcing someone to do something.

So comprehensive, so total, was the Spanish response to the challenge of the New World that the freedom of the Indian in the Spanish colony became, as everyone knows, severely limited. Indians were expected to conform to Spanish norms, to convert to Christianity, to take their place as subjects of the Spanish king. In the phrase so frequently repeated during the sixteenth

²⁵ Zurita y Castro, *Anales*, 6: 222.

²⁶ *CDIHE*, 8: 457.

century, the Indians of America were declared to be “free vassals” of the crown. But observe some of the implications of this. The conquests and subsequent institutional controls provided little opportunity for Indians to *declare themselves* to be vassals, free or unfree, or even to understand what vassalage entailed. This modified at least the theoretical basis of historic vassalage, which in Spain and elsewhere, and perhaps more in Spain than elsewhere, depended upon agreement, fealty, homage, honor, a sense of duty, willing service—in short attitudes incapable of being coerced.²⁷

Paradoxical as it may be, Spaniards did sometimes insist that the Indians of America had become vassals of the Spanish crown not through conquest or compulsion but through the Indians’ own decision. A number of documents make the point explicitly. One was the famous Requirement of 1513, which notified Indians beforehand that they had to choose, and incidentally outlined the dire consequences for the future if they chose wrongly. Students have emphasized the bizarre features of this Requirement. But when we have finished with its absurdity, we may note that its underlying philosophy on questions of volition, punishment, and enslavement—if not Christian conversion—is that of King Ferdinand at Málaga. The Requirement stands as a kind of generalized capitulation or *pacto* in advance for American Indians: voluntary vassalage will bring good results; continued resistance will bring slavery.²⁸

Montezuma’s concession of the Aztec Empire to Charles V provides us with another illustration of the principle. Cortés and his fellow conquistador, Bernal Díaz del Castillo, are the primary authorities for Montezuma’s formal proclamation of subordination and transfer of Aztec territory to the king.²⁹ That it was a voluntary act became one of the themes of Spanish imperial law and justificatory literature. “Montezuma placed himself under our authority by his own volition,” reads a statement by Philip II in 1577,³⁰ and similar assertions echo through the centuries to the end of colonial times. In addition to their expression of theoretical voluntarism and their deprecation of force, such statements had a bearing on an important and practical subject, namely Indian tribute, which was exacted ostensibly in token of Indian vassalage and was one of the chief sources of royal revenue from America.³¹

Could it be that the supposed freedom of Indians had to be so meticulously recorded by Spaniards in part because there were no Spanish-Indian treaties

²⁷ In one form of Spanish *señorio*, vassals were entitled to change their lord once every twenty-four hours.

²⁸ Lewis Hanke, “The ‘Requerimiento’ and Its Interpreters,” *Revista de historia de América*, 1 (1938): 25–34. Annie Lemistre, “Les origines du Requerimiento,” *Mélanges de la Casa de Velázquez*, 6 (1970): 161–209.

²⁹ See the sources cited and the perceptive comments of Elliott, *The Old World and the New*, 84–85. Bernal Díaz del Castillo, *Historia verdadera de la conquista de la Nueva España*, 2 vols. (Madrid, 1942), 1: 371–73, gives an affecting and detailed account of the incident. Subsequent renunciations by the “heirs” of Montezuma are examined by Silvio Zavala, *Las instituciones jurídicas en la conquista de América* (Mexico, 1971), 319 ff.

³⁰ *Colección de documentos inéditos relativos al descubrimiento, conquista y organización de las antiguas posesiones españolas de América y Oceanía, sacados de los archivos del reino, y muy especialmente del de Indias* (title varies), 42 vols. (Madrid, 1864–84), 6: 67–68.

³¹ E.g., Fabián de Fonesca and Carlos de Urrutia, *Historia general de real hacienda*, 6 vols. (Mexico, 1845–53), 1: 412.

to make the point? For it is in the nature of treaties that they express, or purport to express, agreements willingly entered into by both sides. Diplomacy is the give and take of wills in anticipation of such agreements, and though the will of one may be stronger than that of the other the assumption in the treaty is that a voluntary element remains and receives expression. The treaties that Portuguese, Dutch, French, and English made with Indians, for all the inequalities of strength on one side and weakness on the other, characteristically gave token recognition to Indian volition. We might hypothesize that the treaty was a preferred device of those nations in dealing with Indians precisely because of this fact, precisely because it bespoke an Indian free will that was, in realistic modern terms, extremely limited or even nonexistent. As the traditional instrument of negotiated accord, the treaty disguised but did not otherwise modify the strength of one side and the weakness of the other. In the imperial policies of other nations the treaty became the standard means for pretending that the niceties of international relations were being respected between whites and natives. In the Indian treaties of the United States after independence, as one would expect, there is a less prolix insistence on the defeated party's freedom of choice than in the Spanish capitulation cases noted above. But the pretended assumption of Indian volition is there also, and the irony, dismay, and guilt attendant on this surely help to explain the Congressional rejection of Indian treaty-making and the substitution of a new system in the United States in 1871.³² What I am suggesting is that the treaties did perform, up to a point, the important function of expressing a supposed Indian volition and that in the absence of treaties Spaniards had to assert that volition in other ways.

A number of other events in the history of Spain in America might be cited to illustrate Spanish attitudes toward free will and necessity. Frequently they would not coincide with our own attitudes on these subjects, and that is also my point. Attitudes do change from one historical period to another. There would seem to be all the difference in the world between what we choose to do and what we are compelled to do. Yet the same act can appear to one age to be the one and to a subsequent age to be the other. Those historians who have identified the basic changes in attitude from one age to the next are rightfully regarded as masters of our trade. But I would have to say that, for the periods and places of history that I have studied, we are still a long way from sorting out, from truly understanding, the differences between how people thought in the former time and how we think now.

I believe that you are not expecting, and are not prepared for, my concluding statement, which is that I have lately learned of two genuine treaties between Spaniards and Indians. Both are from the United States borderlands in the late eighteenth century. One is with the Creeks in West Florida signed at Pensacola on June 1, 1784. The other is with the Alibamos, Chickasaws,

³² Charles J. Kappler, ed., *Indian Affairs, Laws, and Treaties*, 3 vols. (Washington, 1903-13).

and Choctaws, signed at Mobile three weeks later.³³ The two treaties have a derived and non-Hispanic look, as if the Spanish nation, in the period of her full decline, found it necessary to adopt her enemies' devices. Perhaps there were other treaties than these two, here in Texas or elsewhere in Spanish America, and if any of you know of others you are invited to notify me of them. If many more appear, we could attempt a tabulation, to juxtapose with that of the Granada capitulations for whatever the comparison may yield. But if too many appear I should have to retract much of what I have said this evening. Retraction would compound the consequences of failure. But I would still hope that it would be a good failure, one worth making and reporting. Between Granada and Texas we may still have learned something about conquests, capitulations, attitudes toward volition, and our other subjects. Besides, we have never quite denied that Spaniards may have made treaties with Indians. In our investigations into history, should we not always grant to the people and things we study the freedom, or volition, to take us and our audience by surprise?

³³ I am grateful to Peter Zahendra for this information. For additional details, see his "Spanish West Florida, 1781-1821" (Ph.D. dissertation, University of Michigan, 1976), 188-90, 228.

Iberian Expansion and the Issue of Black Slavery: Changing Portuguese Attitudes, 1440–1770

A. J. R. RUSSELL-WOOD

IN 1441 ANTÃO GONÇALVES, the young captain of a small vessel dispatched by the infante Dom Henrique of Portugal to take on a cargo of seal skins and oil, put ashore a landing party on the coast of northern Mauretania for the express purpose of capturing blacks. After light skirmishing one black male and one black female were taken prisoner. Shortly afterwards, Gonçalves was joined by Nuno Tristão, a knight of the infante's household, who had been commissioned to sail his armed caravel as far as possible down the coast of West Africa and take captives. The two captains combined their forces to assault coastal villages by night and succeeded in taking ten more captives. On their return to Portugal they paraded their trophies before an appreciative Dom Henrique.¹ Initial Portuguese reaction was cool, but entrepreneurial and commercial instincts were soon fanned by the arrival in the Algarve of further consignments of blacks from the sub-Saharan region. Lançarote da Ilha, collector of royal taxes (*almoxarife*) in the port of Lagos, gained the infante's permission to organize a trading expedition to Mauretania. Six caravels under Lançarote's general command left Lagos with no other objective than the capture of what were described as "black Moors." On August 8, 1444 some 235 blacks were unloaded at Lagos.² This event heralded a new era in the social, economic, and ideological history of Portugal, and its legacy was to survive the separation in 1822 of Portugal's largest and richest colony—Brazil.

The arrival of this cargo of black slaves was hardly the first encounter of the Portuguese with the institution of slavery. Slavery had declined in the Iberian peninsula after the Visigothic era, only to increase in the ninth century with

I wish to acknowledge my appreciation to Lewis Hanke and Philip D. Curtin for their valuable suggestions based on the reading of an earlier draft of this paper.

¹ Gomes Eanes de Zurara's *Crônica dos feitos de Guiné*, chaps. 12–14, is still the basic source for this account. The first English translation and edition by C. R. Beazley and E. Prestage—*The Chronicle of the Discovery and Conquest of Guinea* (Hakluyt Society Publications, 1st. ser., 95, 100; London, 1896, 1899)—has been superseded by the definitive edition of Léon Bourdon in collaboration with Robert Ricard: Gomes Eanes de Zurara, *Chronique de Guinée* (Mémoires de l'Institut Français de l'Afrique Noire, no. 60; Ifan-Dakar, 1960).

² Zurara, *Crônica dos feitos de Guiné*, chaps. 18–25. Zurara placed the final figure at 235 but the tally of individual raids amounts to 240. For a discussion of this discrepancy and different figures advanced by João de Barros and Bartolomé de las Casas, see Zurara, *Chronique de Guinée*, ed. Bourdon, 17, n. 3; 108, n. 3.

greater numbers of Moors. Enslavement of Muslims captured in battle was but one of the social repercussions of the lengthy Christian reconquest of those parts of the peninsula which had fallen under Moorish domination. The number of captives taken depended on the intensity and success of the Christian campaigns.³ After the establishment of Portugal as an autonomous kingdom, the efforts to oust "infidels" from the national territory intensified. Dom Afonso Henriques, the count of Portugal who used the royal title some forty years before officially being recognized as king in 1179, took advantage of internal discord within Iberian Islam to score a decisive victory over the Moors at the battle of Ourique in 1139 and to capture Santarém and Lisbon in 1147. As the Almohad empire gradually collapsed in the second quarter of the thirteenth century, Christian forces advanced southwards from the Alentejo. This advance culminated in the capture of Silves and Faro in the southern Algarve in 1249 by Dom Afonso III (king of Portugal, 1248-79).

Although not officially designated as such, these Portuguese campaigns were imbued with the spirit of crusade. In his conquest of Lisbon, Dom Afonso Henriques enjoyed the assistance of French, German, Flemish, and English soldiers of the Second Crusade on their way to Palestine. Soldiers of the Third Crusade were present at the storming of Silves in 1189. Dom Afonso II (king of Portugal, 1211-23) enlisted the help of soldiers of the Fifth Crusade at the capture of Alcácer do Sado. The newly founded military orders of Calatrava (ca. 1156) and Santiago (ca. 1170) played a prominent role in the Algarvian campaigns of Dom Sancho II (king of Portugal, 1223-48). Dom Afonso IV (king of Portugal, 1325-57) regarded as both a personal and a national crusade his joint victory with the Castilian forces of Alfonso XI over the Muslims on the banks of the river Salado in Andalusia in 1340. Whether God, gold, or greed provided the incentive for Portuguese expansion, the expedition against Ceuta in 1415 and the disastrous attack against Tangier in 1437 represented the first concerted offensives by Portuguese against Muslims beyond the frontiers of Europe. Enterprises such as these in Morocco provided Portugal with a steady flow of Moorish slaves.⁴

Against this background of religion, war, and slavery, the significance of Antão Gonçalves' action must be assessed. The arrival of blacks marked a break with longstanding tradition, not merely because the Africans came from a different geographical location and had a different ethnic heritage but because their arrival engendered the development of a new attitude toward the institution of slavery. Previous slaving activity on the African coast and the Atlantic islands consisted of sporadic raids by armed groups composed of many nationalities. Portuguese had landed shore parties on the Canary Islands to obtain *Guanches* for labor on the sugar plantations of Madeira.

³ Charles Verlinden, "L'esclavage dans le monde ibérique médiéval," *Anuario de historia del derecho español*, 11 (1934): 283-448, and his monumental *L'esclavage dans l'Europe médiévale*, vol. 1: *Péninsule Ibérique-France* (Bruges, 1955).

⁴ Robert Ricard, *Études sur l'histoire des Portugais au Maroc* (Coimbra, 1955), and his earlier article, "Le commerce de Berbérie et l'organisation économique de l'empire portugais aux XV^e et XVI^e siècles," *Annales de l'Institut d'Études Orientales*, 2 (1936): 266-90.

Vessels cruising off the Moroccan coast had kept a weather eye open in the hope of abducting Moors from villages. Both sources had been unsatisfactory. The Canaries proved unreliable sources of supply and attacks on Moroccan coastal villages were fraught with danger. Gonçalves' initiative, coupled with Lançarote's commercial acumen, introduced three new elements in the slave trade: (1) discovery of conditions in the sub-Saharan region conducive to establishing a permanent slave mart; (2) enslavement of peoples with whom the Portuguese had no territorial dispute or religious rivalry; and (3) use of black slave labor in Portugal.⁵ Within a few decades the pattern of trade on the West African coast and in the Atlantic islands was irrevocably altered. With the discovery and exploitation of the Americas, slavery and the black man and woman became essential parts of the checkered tapestry of European colonization of the New World. Half a millenium was to elapse before this institutional legacy was exhausted.

ALTHOUGH THE PORTUGUESE RETAINED the lion's share of the slave trade, it was not an exclusively Portuguese prerogative. Not surprisingly in view of Italian contributions to Portuguese discoveries in the fourteenth century and the presence of an active Italian colony in Lisbon, the Genoese were particularly prominent in the West African slave trade.⁶ About 1455 the Genoese Antoniotto Usodimare bought thirty-one slaves directly from a chief on the Gambian coast. In 1486–87 a Florentine merchant, Bartolomeo Marchione, paid 6,300,000 réis for a license to trade to the Slave Rivers (Rio dos Escravos); later his license was renewed and his activities were duly recorded in the Slave House (Casa dos Escravos) newly established by the crown in Lisbon.⁷

Castile presented a greater challenge than Genoa to Portuguese monopoly of the slave trade. The first Castilian voyages to West Africa date from 1453–54. Between 1453 and 1479 sailors and traders took advantage of the proximity of Andalusian ports to Africa to trade directly on the Guinea coast. This trading rivalry only ended in 1479 with the treaty of Alcáçovas. Castile

⁵ M. A. Hedwig Fitzler theorized that Lançarote's enterprise could be considered as an early example of a joint-stock company: "Überblick über die portugiesischen Überseehandelsgesellschaften des 15.–18. Jahrhunderts," *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, 24 (1931): 282–98, and "Portugiesische Handelsgesellschaften des 15. und beginnenden 16. Jahrhunderts," *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, 25 (1932): 209–50. For a rebuttal based on careful evaluation of the "evidence," see Virginia Rau and B. W. Diffie, "Alleged Fifteenth-Century Portuguese Joint-Stock Companies and the Articles of Dr. Fitzler," *Bulletin of the Institute of Historical Research*, 26 (1953): 181–99.

⁶ On the Italian role, see Charles Verlinden, "Italian Influence in Iberian Colonization," *Hispanic American Historical Review*, 33 (1953): 199–211, "La colonie italienne de Lisbonne et le développement de l'économie métropolitaine et coloniale Portugaise," *Studi in onore di Armando Sapori*, 1 (Milan, 1957): 615–28, and "Navigateurs, marchands, et colons italiens au service de la découverte et de la colonisation portugaise sous Henri le Navigateur," *Le Moyen Âge: Revue d'histoire et de philologie*, 4th ser., 13 (1958): 467–97; Virginia Rau, "A Family of Italian Merchants in Portugal in the XVth Century: The Lomellini," *Studi in onore di Armando Sapori*, 715–26; and Robert Ricard, "Contribution à l'étude du commerce génois au Maroc durant la période portugaise (1415–1550)," *Annales de l'Institut d'Études Orientales*, 3 (1937): 53–73, reprinted in his *Études sur l'histoire des Portugais au Maroc*, 115–42. Despite this participation, a recent study notes that black slaves were "un settore del tutto irrilevante" in the city; see Domenico Gioffrè, *Il mercato degli schiavi a Genova nel secolo XV* (Genoa, 1971), 33–36, 59.

⁷ Verlinden, *Péninsule Ibérique-France*, 624–27.

surrendered all claims to trading rights in Guinea or on the Gold Coast and recognized Portuguese possession of the Azores, Cape Verdes, and Madeira in return for recognition by Portugal of Castilian dominion over the Canary Islands.⁸ Black slaves from Portugal reached Spanish territories both by sea and by land. Although they inhabited Mallorca, Castile, Catalonia, Valencia, Granada, and Aragón, they were most evident in the towns and cities of Andalusia. A document of 1565 records that slaves comprised 3.4 percent of the total population of the Archbishopric of Seville (incorporating the present provinces of Seville and Huelva and parts of Málaga and Cádiz). In 1565 there were well over six thousand slaves in the city of Seville—one tenth of the population.⁹ Toward the end of the sixteenth century, the easy availability of black slaves in Cádiz forced market prices down and provided merchants with the barest of profit margins. In 1616 there were some five hundred black and three hundred Moorish slaves in Cádiz. The existence of a branch of the lay brotherhood of Our Lady of the Rosary for blacks shows that they had achieved a degree of cohesion.¹⁰ Blacks appear in the works of Lope de Vega, Miguel de Cervantes, and other luminaries of the “golden age” of Spanish literature.¹¹

In 1443 Prince Regent Dom Pedro of Portugal had granted to Dom Henrique that fifth of the proceeds derived from the West African trade that was normally reserved for the king. Dom Pedro had taken this unusual step in recognition of expenses incurred by the Order of Christ (founded in 1319 by King Dom Diniz), of which the prince was governor and administrator. Furthermore, voyages south of Cape Bojador could only be made with Dom Henrique's permission.¹² Such permission was readily granted—with the proviso that part of the profits be deposited in the coffers of the order. According to the Portuguese chronicler Gomes Eanes de Zurara (ca. 1420–73/74), 927 slaves had been brought from the Saharan coast of Guinea to Portugal in the period 1441–48. The modern Portuguese historian Vitorino Magalhães

⁸ John William Blake, *European Beginnings in West Africa, 1454–1578* (New York, 1937), 16–56, and Blake, trans. and ed., *Europeans in West Africa, 1450–1560: Documents to Illustrate the Nature and Scope of Portuguese Enterprise in West Africa, the Abortive Attempt of Castilians to Create an Empire There, and the Early English Voyages to Barbary and Guinea* (Hakluyt Society Publications, 2d. ser., vols. 86–87; London, 1942), esp. 1: 185–246; and Vitorino Magalhães Godinho, *Os descobrimentos e a economia mundial*, 2 (Lisbon, 1965): 548–49. For an English translation of the treaty, see Charles Gibson, ed., *The Spanish Tradition in America* (New York, 1968), 17–26.

⁹ Antonio Domínguez Ortiz, “La esclavitud en Castilla durante la edad moderna,” *Estudios de historia social de España*, 2 (1952): 376–79, and *Orto y ocaso de Sevilla* (2d ed.; Seville, 1974), 102–05; and Ruth Pike, *Aristocrats and Traders: Sevillian Society in the Sixteenth Century*, (Ithaca, 1972) 170–71, and “Sevillian Society in the Sixteenth Century: Slaves and Freedmen,” *Hispanic American Historical Review*, 47 (1967): 344–59. On black slaves in Valencia, see Vicenta Cortes, *La esclavitud en Valencia durante el reinado de los Reyes Católicos, 1479–1516* (Valencia, 1964), 56–59.

¹⁰ Godinho, *Os descobrimentos e a economia mundial*, 543–44; and Hipólito Sancho de Sopranis, “Estructura y perfil demográfico de Cádiz en el siglo XVI,” *Estudios de historia social de España*, 2, (1952): 602–03, and *Las Confradías de Morenos en Cádiz* (Madrid, 1958).

¹¹ See references under “esclavo” and “negro” in Carlos Fernández Gómez, *Vocabulario de Cervantes* (Madrid, 1962), and in *Vocabulario de Lope de Vega*, 2 vols. (Madrid, 1971). Also see Ricardo del Arco y Garay, *La sociedad española en las obras dramáticas de Lope de Vega* (Madrid, 1941), 618–23; and John Brooks, “Slavery and the Slave in the Works of Lope de Vega,” *Romanic Review*, 19 (1928): 232–43.

¹² Order, dated October 22, 1443; it has been published in João Martins da Silva Marques, *Descobrimientos portugueses: Documentos para a sua história, 1147–1460* (Lisbon, 1944), 435–36. Also see Zurara, *Crónica dos feitos de Guiné*, chap. 15.

Godinho regards this estimate as too conservative; the figure should be between one and two thousand.¹³ In 1448 a trading fort was erected on the island of Arguin and regular commerce was established. The Portuguese acquired slaves by incursions inland or by purchase on a regular basis from Muslim or black traders. Portuguese attacks on coastal villages gave way to barter and other more peaceful methods of exchange. In many instances the Portuguese merely continued traditional patterns of commerce established by Muslim traders.¹⁴

During the 1450s trade intensified. The Italian Alvise de Cà da Mosto, who voyaged to the West African coast from 1455 to 1457, reported that annual Portuguese exports ranged between seven and eight hundred slaves. Building the fort and trading post at Elmina (São Jorge da Mina) in the Gulf of Guinea in 1482 and establishing Portuguese interests in the Congo placed trade relations on a firm footing. Increased commerce demanded greater administrative oversight in Portugal, and in 1486 the crown created the Casa dos Escravos de Lisboa (Lisbon Slave House) as a subsidiary of the Royal Guinea House. Nor did the Portuguese ignore the potential of the Atlantic island of São Tomé as a source of slaves. A royal letter of December 11, 1493 encouraged trade in slaves between São Tomé and Elmina. Any balance sheet for the second half of the fifteenth century must be tentative because of incomplete data, but, according to the estimates of Magalhães Godinho, between one and two thousand slaves were exported during the years 1441–48. Arguin alone exported at least twenty-five thousand in the period 1450–1505. To the south of Senegal, the trade probably did not exceed five hundred slaves annually between 1450 and 1460, but it doubled in the next decade and continued to grow during the last quarter of the fifteenth century. Slaves originating from the region between Arguin and Sierra Leone, who entered Lisbon and the ports of the Algarve between 1441 and 1505, totalled between one hundred and forty and one hundred and seventy thousand. In the sixteenth century total exports from West Africa to Portugal numbered some three hundred thousand slaves.¹⁵

Not all blacks arriving in Lisbon and the Algarve remained in Portugal. Portugal carried on an active slave trade with Aragón, Castile, Catalonia, Valencia, Andalusia, and the Canary Islands as well as with states north of the Pyrenees. Those slaves remaining in Portugal were employed primarily on

¹³ Zurara, *Crónica dos feitos de Guiné*, chap. 96. Duarte Leite, in *Acerca da "Crónica dos feitos de Guiné"* (Lisbon, 1941), 161–62, places the figure at some 850 slaves, yet he cites a letter of Dom Afonso V of September 25, 1448 that refers to "more than one thousand unbelievers taken captive." Also see Zurara, *Chronique de Guinée*, ed. Bourdon, 265, n. 1; and Godinho, *Os descobrimentos e a economia mundial*, 524–25.

¹⁴ For general studies of this period, see Blake, *Europeans in West Africa, 1450–1560*, 3–181; João Lúcio de Azevedo, *Épocas de Portugal económico: Esboços de história* (Lisbon, 1929), 59–87; P. E. L. R. Russell, *Prince Henry the Navigator* (London, 1960); Godinho, *Os descobrimentos e a economia mundial*, 517–55; and C. R. Boxer, *The Portuguese Seaborne Empire, 1415–1825* (London, 1969), 15–38.

¹⁵ Godinho, *Os descobrimentos e a economia mundial*, 524–39; Verlinden, *Péninsule Ibérique-France*, 616–29; Boxer, *The Portuguese Seaborne Empire, 1415–1825*, 31; and Philip D. Curtin, *The Atlantic Slave Trade: A Census* (Madison, Wis., 1969), 17–21. On the Casa dos Escravos, see the well-documented study by John L. Vogt, "The Lisbon Slave House and African Trade, 1486–1521," *Proceedings of the American Philosophical Society*, 117 (1973): 1–16.

the sugar plantations of Madeira, in agriculture generally, and in domestic service. Some plied their skills as artisans. In 1554 one slave was a naval carpenter.¹⁶ No reliable figures are available for the number of blacks in Portugal in the fifteenth and sixteenth centuries. The term “slave” is itself misleading, inasmuch as it was also applied to those not of sub-Saharan origins. In Seville Moorish and Morisco slaves had been described as “white slaves.” In 1460 a municipal decree in Lisbon distinguished between “black and white slaves.”¹⁷ For a few years at the end of the fifteenth century, Jews were reduced to slavery. Dom João II permitted Jews to enter Portugal—conditional on payment of eight *cruzados* at the frontier and a sojourn in Portugal of not more than eight months—after their expulsion from Spain in 1492. Failure to meet these conditions resulted in enslavement. In 1495 Dom Manuel ordered the liberation of Jews who had been victims of this draconic ruling. With Portuguese expansion into the Indian Ocean and South China Sea and to the Americas, Asiatic and Amerindian slaves were sent to Portugal. In 1534 Duarte Coelho, donatory of the captaincy of Pernambuco in northeast Brazil, received royal authorization to import twenty-four Amerindians annually into Portugal through the port of Lisbon.¹⁸ In the influx of exotica flooding Lisbon in the fifteenth and sixteenth centuries, which included spices, ivory, gold, silks, porcelain, and even a rhinoceros and a lion, blacks were merely one more item. Yet, although blacks from West Africa formed but part of the total slave population, they attracted the attention of visitors to Portugal.

One such visitor was a Bohemian baron, Leo of Rozmítal, in 1466. Honored by an audience with Dom Afonso V, who was known as “the African” because of his Moroccan campaigns, Rozmítal received two apes, two horses, and two Moorish slaves from the king. The baron commented on large numbers of “Ethiopian” slaves in Portugal and observed that black slaves were sold by the thousand each year in Oporto.¹⁹ It appears, however, that the majority of black slaves remained in and around Lisbon, which had replaced Lagos as the major port in the slave trade. The first census for Portugal dates from 1527, but it is of no assistance in assessing the black component in the population: although the number of hearths (*fogos*) recorded for the national territory was 280,582, the figure is virtually meaningless in demographic terms because a “hearth” could refer equally to a manor house, a convent, or a widow’s croft. The Portuguese geographer Orlando Ribeiro has suggested that black slaves may have comprised one tenth of the population of Lisbon in 1527. As such, they would not have numbered more than some five to six

¹⁶ Verlinden, *Péninsule Ibérique-France*, 838.

¹⁷ Pike, *Aristocrats and Traders*, 171; and Fortunato de Almeida, *História de Portugal*, 6 vols. (Coimbra, 1922–29), 3: 223. On Jewish slaves in Spain, see Domínguez Ortiz, “La esclavitud en Castilla,” 376.

¹⁸ Letter of grant, March 10, 1534 (Évora), translated into English by E. Bradford Burns in his *A Documentary History of Brazil* (New York, 1966), 34–45.

¹⁹ Malcolm Letts, ed. and trans., *The Travels of Leo of Rozmítal through Germany, Flanders, England, France, Spain, Portugal, and Italy, 1465–1467* (Hakluyt Society Publications, 2d ser., 108; Cambridge, 1957), 106–07, 110, 118.

thousand.²⁰ In 1516 the chronicler, poet, and musician Garcia de Resende observed that slaves in Portugal would, “in my opinion, soon be more numerous than we.”²¹ In the 1530s the Belgian grammarian and traveller Nicolas Cleynaerts—tutor to Dom Henrique, who was the brother of Dom João III—wrote, “Slaves swarm everywhere. All work is done by blacks and captive Moors. Portugal is crammed with such people. I should think that in Lisbon slaves, male and female, outnumber freeborn Portuguese.” And, he continued, “Richer households have slaves of both sexes, and there are individuals who derive substantial profits from the sale of the offspring of their household slaves. In my view they raise them much in the same way as one would raise pigeons for sale in the marketplace.”²²

Cleynaerts exaggerated in this claim, as did Duarte Nunes de Leão, who in his *Descrição de Portugal* (1559) referred to hundreds of slaves from Guinea, Ethiopia, and India in Portugal. In 1551 Cristóvão Rodrigues de Oliveira, a more reliable witness, calculated the population of Lisbon at almost one hundred thousand: more than eighty thousand Portuguese, over seven thousand foreigners, and close to ten thousand black slaves. In 1552 there were twelve slave markets in the nation’s capital; and by 1573 there were some forty thousand slaves in Portugal. There may have been an overall decline at the end of the sixteenth century, which continued into the seventeenth. In 1620 Friar Nicolau de Oliveira opined that black slaves in Lisbon numbered roughly ten thousand five hundred out of a total population of approximately one hundred and sixty-five thousand. If these figures are reasonably accurate, they indicate a drop in the number of black slaves in Lisbon from slightly under 10 percent in 1551 to 6.3 percent in 1620. Magalhães Godinho considers these figures too conservative, but accepts a general decline in the Portuguese black slave population, which was more concentrated in the cities of Oporto, Lisbon, and the Algarve than elsewhere.²³

Despite their proportionately small number, black slaves had considerable impact on the policies and attitudes of a people still imbued with a medley of concepts inherited from the late Middle Ages. Concomitant with discoveries and settlements in Africa, Asia, and the Americas, black slavery compelled the Portuguese to reassess values that had previously gone unchallenged. Among these values were the notions of “honor” and “just war.”

²⁰ Orlando Ribeiro, *Portugal*, vol. 5 of *Geografia de Espanha y Portugal*, gen. ed. Manuel de Terán (Barcelona, 1955), 99–101.

²¹ “Vemos no Reyno meter / tantos captiuos crescer, / e irense hos naturaes, / que se assi for, serão mais / elles que nos, a meu ver,” as quoted in Almeida, *História de Portugal*, 3: 218.

²² M. Gonsalves Cerejeira, *O Renascimento em Portugal-Clenardo*, 2 (Coimbra, 1918): apps. 14–15, as cited by Almeida, *História de Portugal*, 3: 219.

²³ For a general discussion, see Godinho, *Os descobrimentos e a economia mundial*, 538–42. For the slave marts, see Lúcio de Azevedo, *Épocas de Portugal económico*, 75. Although parochial studies for Lisbon—E. Prestage, *Registro da freguesia de Santa Cruz do Castello desde 1536 até 1628* (Lisbon, 1913); and E. Prestage and Pedro d’Azevedo, *Registro da freguesia da Sé desde 1563 até 1610 (óbitos, casamentos, batizados)*, 2 vols. (Coimbra, 1924–27)—have appeared, a comprehensive study of blacks in Portugal has not been undertaken. For an introduction to the topic, however, see António Brásio, *Os pretos em Portugal* (Lisbon, 1944).

IN THE FIFTEENTH CENTURY THE PORTUGUESE NOBILITY experienced financial hardship and lost that position of social pre-eminence to which its members had become accustomed. Their privileges were challenged and, in some instances, curtailed. But they still enjoyed social and financial prerogatives of importance. They were, for example, exempt from certain taxes, and troops could not be billeted on their property. But perhaps the single prerogative most cherished by the nobility was the right of access, by virtue of deeds of prowess, to the gaining of "honor."²⁴

The nature of "honor" was best described by the infante of Portugal, Dom Pedro (1392–1449), in his treatise *Tratado da Virtuosa Benfeitoria*, dedicated to the Virgin Mary and largely translated from Seneca's *De beneficiis*. The quintessence of "honor" lay in disinterest. The nobleman in search of "honor" upheld spiritual values, morality, and the purity of justice. He was the defender of the Catholic faith and protector of the needy. These roles endowed the noble with a degree of spiritual grace denied to all not of the same social rank and order. He gained not only the respect of men but also the gratitude of his sovereign. Titles, pensions, and gifts conferred by the crown were tokens of royal esteem rather than rewards for services performed. Whereas the nature of deeds of prowess remained undefined, there was no doubt but that the supreme act of vassalage was in the service of God. This did not mean mere obedience to the precepts of Catholicism; the noble was obligated to spread Catholicism or, conversely, to halt and turn back the influence of Islam. Dom Pedro's brother, the infante Dom João, expressed the widely held view that "honor" could only be acquired by deeds of valor and exposure to personal danger in the service of king or God on the field of battle.

The concept of "just war" was to bedevil Portuguese and Spanish theologians, jurists, and monarchs both in their domestic policies and in their treatment of the inhabitants of territories in Africa, Asia, and the Americas over whom they exerted dominion. On the Iberian peninsula the concept of "just war" had been applied to offensive actions by Christians against Moors. Catholic sovereigns and their nobles generally agreed that the "service of God" not only brought "honor," but placed justice on the side of the aggressor. They left to theologians the task of determining those criteria which differentiated a "just" and an "unjust" conflict. Thomas Aquinas specified the three main conditions for "just war": right authority, sufficient cause, and right intention. Of paramount importance was right authority, and the legitimacy of war in the name of Christ was difficult to deny. Thus, the outcome depended primarily on the religious beliefs of the enemy. Although "just wars" among Christians were not unknown, theologians preferred to apply the term to wars conducted by Christians against non-Christians. In his *Summa theolog-*

²⁴ For an excellent analysis of the concept of honor, see António José Saraiva, *História da cultura em Portugal*, 3 vols. (Lisbon, 1950–62), 1: 571–82. Also see Margarida Barradas de Carvalho, "L'idéologie religieuse dans la 'Crónica dos Feitos de Guiné' de Gomes Eanes de Zurara," *Bulletin des Études Portugaises et de l'Institut Français au Portugal*, 19 (1955–56): 34–63.

ica Aquinas conceded that unbelievers should not be compelled to embrace Catholicism by force of arms. But he equally emphasized that such unbelievers should not be permitted to impede the diffusion of Catholicism.²⁵ Jurists and later theologians supplied the unavoidable *sequitur* to such reasoning: since unbelievers by their very existence could scarcely do other than impede proselytism, any attack on unbelievers by a Christian force was justified.

During the centuries of reconquest no theologian or sovereign in the Christian kingdoms of the Iberian peninsula doubted that the repeated offensives against the followers of Islam constituted “just war.” In Portugal completion of the reconquest by the mid-thirteenth century and the virtual absence of further interreligious strife changed attitudes toward Moors and Jews and diminished the importance of crusader ideology. Attacks on Moroccan citadels proposed by Dom João I (king of Portugal, 1385–1433) brought the issue of “just war” to the fore again. The religious opponents had not altered, but the geographical locale had. No longer could it be argued that the cities, towns, and religious sanctuaries of Portugal were threatened by the infidel. Dom João convoked theologians to address the question of whether or not such a Portuguese offensive beyond the frontiers of Continental Europe would be in the “service of God” and thus fall within the category of “just war.” In concluding that attacking Morocco was justified, the theologians gathered not only scriptural references and precepts of canon law but also invoked the heroic traditions of the Iberian peoples in their struggles against Islam. The theologians ruled that the African continent had formerly been a Christian enclave, that Muslims had occupied this territory by force, and that they had profaned Christian places of worship. War against Morocco would be the first step in restoring to Catholics their religious patrimony. Thus, it was a “just war,” and, as such, all captives taken in the campaign could legitimately be reduced to bondage. Evoking a longstanding Iberian tradition of heroism, Dom João and his soldiers were depicted as heirs to a legacy of resistance ranging from the Goths to the Cid, including the miraculous victory of Dom Afonso Henriques at Ourique against numerically superior Moorish forces and the no less incredible victory of Dom Afonso IV at the battle of the Salado. The Portuguese sought papal sanction for the expedition from the antipope, John XXIII, with whom the crown enjoyed good relations. The pontiff granted a bull of indulgence for the expedition. On July 28, 1415—as the fleet lay in the roadstead of Lagos prior to departure—the royal confessor, Friar João Xira, administered collective absolution to the participants. For pope, king, nobles, and soldiers the expedition to Ceuta was tantamount to a crusading expedition, which Charles Gibson has characterized as the “first act of state-directed imperialism of modern European history.”²⁶

²⁵ Joan D. Tooke, *The Just War in Aquinas and Grotius* (London, 1965), 21–29; and *The “Summa theologica” of St. Thomas Aquinas Literally Translated by Fathers of the English Dominican Province*, 22 vols. (London, 1913–42), part 2 (second part), 9: q. 10, art. 8.

²⁶ Saraiva, *História da cultura em Portugal*, 1: 582–92; Charles Martial de Witte, “Les bulles pontificales et l’expansion portugaise au XV^e siècle,” *Revue d’histoire ecclésiastique*, 48 (1953): 683–718; and Charles Gibson, *Spain in America* (New York, 1966), 2. Also see H. V. Livermore, “On the Conquest of Ceuta,” *Luso-Brazilian Review*, 2 (1965): 3–13.

Ceuta was also just the first in a long line of Portuguese expeditions to, and discoveries in, Africa and Asia. Contact with previously unknown religions, institutions, cultures, and peoples forced the Portuguese to reassess such concepts as “honor” and “just war.” Crown policy had to be modified in response to new political and economic developments, for it could no longer be satisfactorily couched in the traditional language of an “ideology of crusade.”

Within Portugal itself, centuries of daily exposure to Moors and Jews had provided Catholics with a basic appreciation of the tenets of Islam and Judaism and the mores of their respective adherents. In neighboring Spain Alfonso X, “the Wise” (king of Castile and León, 1252–84), had ordered the translation of the Koran and the Talmud. In Portugal during the reign of Dom Diniz (1261–1325) a translation from the Arabic had been made of the *Chronicle of the Moor Rasis*, and Christians, Muslims, and Jews collaborated on such translations at the Portuguese court. Moorish musicians were much in demand. On the occasion of the marriage of the infante Dom Afonso in 1471, King Dom João II turned to the *mouraria* of Lisbon for entertainers.²⁷ No longer was the designation “Moor” synonymous with “slave.” As early as 1095 a charter of Santarém had referred to “free Moors.” In 1179 Dom Afonso Henriques bequeathed to the monastery of Santa Cruz all Moors in his possession at the time of his death. Testaments of the twelfth century frequently referred to the manumission of Moorish slaves.²⁸ Indeed, so widespread had been the assimilation of Moorish peoples in Portugal by the fifteenth century that opponents of the Moroccan campaigns followed the lead of Infante Dom Duarte in questioning how such attacks in North Africa could be justified. The coexistence of Muslims and Catholics in Portugal made the Moroccan expedition seem incongruous. Yet the official view—that the incursions into North Africa and even the later Portuguese activities on the Guinea coast represented an extension onto African soil of traditional and justified struggles by Christians against “the infidel”—prevailed.

With the dawn of the so-called “Vasco da Gama era,” the Portuguese confronted a host of “new” religions. These ranged from Buddhism, Confucianism, and Shintoism to a number of African religions whose lack of sacred texts and use of unknown symbols baffled the Portuguese. Vasco da Gama’s *faux pas* at Calicut in 1498 (he fell on his knees to worship what he erroneously took to be statues of the Virgin Mary and Catholic saints in a Hindu temple) exemplified in dramatic fashion the incomprehension felt by many Portuguese.²⁹ Faced by the realization that these previously unknown religions were neither Islam nor Judaism, the Portuguese sailors, soldiers, and merchants in Asia and the king and his courtiers in Portugal concluded that

²⁷ Saraiva, *História da cultura em Portugal*, 1: 72.

²⁸ Verlinden, “L’esclavage dans le monde ibérique médiéval,” 403–06; 407, n. 103; 447–48.

²⁹ For histories in translation by sixteenth-century Portuguese chroniclers who reported this incident, see Fernão Lopes de Castanheda, *The First Booke of the History of the Discoverie and Conquest of the East Indies, Enterprised by the Portingales* (London, 1582), chap. 16; and Jerónimo Osório, *The History of the Portuguese during the Reign of Emmanuel*, trans. James Gibbs, 1 (London, 1752): 63–64. See also Elaine Sanceau, *Good Hope: The Voyage of Vasco de Gama* (Lisbon), 118–19.

such beliefs must be variant or degenerate forms of Catholicism. Portuguese uncertainty was heightened by the confrontation with civilizations, potentates, and commercial empires whose material wealth and splendor greatly exceeded those of Iberia.

As the crown had done prior to the opening of the sea route to India and later did when the Portuguese turned to the settlement and colonization of the Americas, the king turned to theologians and jurists to examine the ramifications of this new phase of overseas conquest and colonization. An all-embracing “ideology of expansion” developed that eased the moral dilemma experienced by individual Portuguese in the tropics.³⁰ Intended to curb criticism of the less edifying aspects of “Portuguese India,” the “ideology of expansion” embraced the concept of an ethnic God, a nationalized deity who provided spiritual strength and physical protection for his chosen people—the Portuguese. In return, the Portuguese assumed a national obligation to defend and, if possible, extend Christendom. At the second siege of Diu in 1546 a musket shot carried away one arm of a cross borne into battle by a Franciscan friar, who exhorted the Portuguese soldiers, “Oh, brothers and sons of Christ, heed this insult wrought on you by these infidels! Fight to the death, and die for your Jesus Christ.” On other fronts the same friar called on the Portuguese to follow “Christ your Captain.”³¹ Miraculous turns of fortune favoring the Portuguese and the miraculous appearance of St. James in the Portuguese line strengthened popular conviction that Christ had chosen the Portuguese to be his flag carrier. Beyond the Cape of Good Hope, Portuguese life-styles were governed more by worldly than by spiritual considerations. Fernão Mendes Pinto—sometime Jesuit novice (1554–56), opportunist, soldier of fortune, trader, “thirteen times a prisoner and seventeen a slave,” who was to be immortalized by William Congreve in *Love for Love* (1695) as the liar *par excellence*—gained notoriety and recognition by his exploits, but he was by no means unique in sixteenth-century Portuguese Asia.³² By virtue of nationality alone, such adventurers invoked *their* Christ for protection in some of the more heinous exploits which marred the Portuguese record in the East.

Elevation of Portuguese activities from the national to the universal plane was an inevitable corollary of this ideology. Isolated campaigns or individual deeds—when undertaken by Portuguese—became imbued with an aura of spirituality and took on global significance. In an age when Christendom was convulsed by internal schisms, Portugal not only became heir to the Iberian tradition of reconquest but also became the self-designated defender of Christianity itself against inroads by unbelievers. The theory of restitution, which

³⁰ An excellent survey of the application of a seigniorial ideology to the vicissitudes of empire is in Saraiva, *História da cultura em Portugal*, 3: 191–496.

³¹ Gaspar Correia, *Lendas da Índia*, 4 vols. (Lisbon, 1858–66), 4, pt. 2: chap. 64.

³² “Ferdinand Mendes Pinto was but a type of thee, thou liar of the first magnitude,” Foresight in *Love for Love*, as cited by Maurice Collis, *The Grand Peregrination: Being the Life and Adventures of Fernão Mendes Pinto* (London, 1949), 298. Also see António José Saraiva, *Fernão Mendes Pinto* (Lisbon, 1958), and the exhaustive study by G. Schurhammer, *Fernão Mendes Pinto und seine Peregrinação* (Leipzig, 1926). For more general studies, see R. S. Whiteway, *The Rise of the Portuguese Power in India, 1497–1550* (London, 1899); and C. R. Boxer, *Fidalgos in the Far East, 1550–1770: Fact and Fancy in the History of Macao* (The Hague, 1948).

had justified the Moroccan campaigns of Dom João I, was extended to the rest of the African continent and to Asia as well. The theory took strength from the legend that St. Thomas had converted the Orient to Christianity. Although there is evidence to support the contention that Christian "East Syrian" traders, who came from the area of the Persian Gulf and followed the Nestorian creed, were present before 550 A.D. in Ceylon and Malabar, there are no grounds for believing that the activities of the Apostle extended beyond southern India. Portuguese acceptance of this legend rested less on historical evidence than on apparent coincidences. One such was the discovery by well-diggers at Goa in 1512 of a small crucifix of an unknown metal. Governor Afonso de Albuquerque immediately sent it to King Dom Manuel, "the Fortunate."³³

Portuguese military presence became justifiable as a deterrent to non-Christians who might otherwise spread doctrinal errors. Portuguese attacks became Christian offensives designed to reduce territory held by non-Christians. Christ ceased to be a mystical entity: He took on the guise of a warrior God in whose service the distinction between good and evil became blurred. Such an ideology justified both initiating military actions and securing strong economic bases. To sustain war against the infidel and to bolster the overall economic fortunes of Christendom mandated a spice trade in Portuguese hands. Pillage, piracy, and wanton destruction were justified as depriving unbelievers of material resources which might otherwise be used against Christianity.

Papal approval which legitimized such attitudes and activities took the form of bulls favoring the Portuguese. The authority to issue such bulls had been the subject of the treatises by Henry of Susa, cardinal bishop of Ostia in the thirteenth century. Prior to the birth of Christ, Susa maintained, heathens had enjoyed the unrestricted right to possess property and rule territory in accordance with the precepts of natural law and *jus gentium*. With the coming of Christ, however, all powers, dominions, and prerogatives had automatically become vested in Him and the pope, who by delegation held the authority to annul all power exercised by infidels and to deprive them of their possessions. With the effective division of the world's population into Christians and heathens, the pope—as the representative of Christ on earth—could vest authority in chosen peoples to subject the non-Christian world to their rule. The Order of Christ was to be a prime beneficiary of such papal favors.

Dom Henrique, as governor of the order, wrung from the papacy concessions which had lasting economic, military, and political importance. They were also to have considerable bearing on the relationship between the Portuguese and the non-Christian and nonwhite peoples. By his bull *Dum diversas* (June 18, 1452) Nicholas V had granted to Dom Afonso V authorization (*facultas*) to attack, conquer, and subdue Saracens and other enemies of Christianity. The king received papal sanction to annex their lands, confiscate

³³ L. W. Brown, *The Indian Christians of St. Thomas* (Cambridge, 1936), 43–91. Correia, *Lendas da Índia*, 2, bk. 2: chap. 40.

their possessions, and reduce them to perpetual servitude. In return for such services in the name of Christ, the king and his followers were granted a plenary indulgence (*plenarium remissionem*). The bull *Romanus pontifex* (January 8, 1455) confirmed many of the provisions of *Dum diversas* but, in addition, granted the Portuguese a virtual monopoly over all conquest, navigation, and commerce from the Maghrib to "the Indies." Sternly admonishing other nations not to infringe in any way upon this prerogative, Nicholas V charged the Portuguese with the spiritual guidance of peoples falling under their domination. Calixtus III issued the bull *Inter cetera* (March 13, 1456), which not only confirmed the authority vested in the Portuguese by his predecessor but also granted to the governor of the Order of Christ spiritual jurisdiction over those regions from Cape Bojador to the Indies that fell under Portuguese hegemony. Common to these bulls is a reluctance to restrict the applicability of such concessions to specific geographical regions. By ambiguous phraseology and generalizations, the popes revealed their hopes for future Portuguese conquests. Conquest and slavery had been legitimized, and the Portuguese extended pontifical prescriptions on Muslim trade within the Mediterranean region to Guinea, Africa, and finally Asia. These and later privileges and exemptions, initially granted by the papacy to the Order of Christ and subsequently to the Portuguese crown after its incorporation of the governorship of the order (1551), constituted the *Padroado Real*, or royal patronage of all missions, religious establishments, and ecclesiastical appointments in Portuguese Africa, Asia, and America.³⁴

The "ideology of expansion" found its official mouthpiece in João de Barros (ca. 1496–1570), the "Portuguese Livy," who was treasurer and then factor (1533–67) of the Casa da Índia e Mina in Lisbon. The draft of his monumental work, *Ásia*, was completed in 1539. Three volumes were published in 1552, 1553, and 1563; the fourth and final volume was edited and published posthumously in 1615. Barros had never ventured beyond Guinea, but his position as official historian to the king and his familiarity with correspondence and reports in the Casa da Índia coupled with his zeal in obtaining Portuguese translations of documents in Arabic and Oriental languages, placed him in a unique position to describe in detail the discoveries, achievements, and reverses suffered by the Portuguese overseas from the first tentative probings along the West African coast to 1538. He was deeply conscious of his role as apologist for royal policies and aspirations. His desire to prove that both the sovereign and his subjects had been motivated only by the loftiest idealism led Barros to disregard the seamier aspects of the Portuguese enterprise.³⁵ Less dependent on royal approval, his contemporary Jerónimo Osório (1506–80) sought equally to imbue the achievements of Dom Manuel and his country-

³⁴ de Witte, "Les bulles pontificales et l'expansion portugaise au XV^e siècle," *Revue d'histoire ecclésiastique*, 49 (1954): 438–61; 51 (1956): 413–53, 809–36; 53 (1958): 5–46, 443–71; and Boxer, *The Portuguese Seaborne Empire, 1415–1825*, 228–48.

³⁵ C. R. Boxer, *Three Historians of Portuguese India: Barros, Couto, and Bocarro* (Macao, 1948); and Manoel Cardozo, "The Idea of History in the Portuguese Chronicles of the Age of Discovery," *Catholic Historical Review*, 49 (1963): 1–19.

men with universal significance. Osório's Latin history of Dom Manuel's reign (*De Rebus Emmanuelis Gestis*, Lisbon, 1571) later earned the praise of poet laureate John Dryden and of Alexander Pope.

Concepts of "honor" and "just war" had been modified and extended to a new age and to peoples and regions previously unknown to the Portuguese. But even such defenders of crown policy and national hopes as Zurara and Barros found it difficult to reconcile the "ideology of expansion" with harsh reality. Zurara questioned the appropriateness of nobles chasing black females along Mauretanian beaches to enslave them. How much "honor," he asked, was involved in such activities? Barros followed Zurara in the treatment of the West African phase of Portuguese discoveries. Rarely critical of the exploits of his fellow countrymen, Barros resorted to euphemisms, dissembling, or omissions in treating the more questionable episodes. Both historians faced the problem of how to avoid equating the quest for "honor" (a noble pursuit) with material gains derived from plunder, looting, and pillage (all plebeian activities). The Dominican missionary and historian, Bartolomé de las Casas (1474–1566), who relied heavily on Zurara and Barros for his own *History of the Indies*, was unsympathetic to their predicament. He observed, "And to be marvelled at is the manner in which the Portuguese historians glorify as illustrious such heinous deeds, representing these exploits as great sacrifices made in the service of God."³⁶

In Portugal itself criticism of national involvement in Africa and Asia was not lacking. Even Barros, in his *Rôpica Pnema* (published in 1532 and listed on the 1581 Index), denounced some tenets fundamental to the "ideology of expansion." Chroniclers who had seen service in the East did not mute their criticism. Fernão Lopes de Castanheda (ca. 1500–59) in his *History of the Discovery and Conquest of India by the Portuguese*, Gaspar Correia (ca. 1492–1565) in his *Legends of India*, and Diogo do Couto (1543–1616) in his *Dialogue by a Veteran Portuguese Soldier* provided a counterbalance to Barros' sensitive selectivity of materials and his exalted style of historiography. Drama and verse also served as vehicles for criticism. Although the court playwright Gil Vicente perpetuated the tradition of "just war" in his *Exortação da Guerra* (1513) and *Auto da Fama* (1515), tongue-in-cheek comments on the negative aspects of Portuguese India are scattered through his works. The poet Sá de Miranda (1481–1558) was outspokenly critical of the moral impact on Portugal and the Portuguese of the "vain airs of the Indies." He compared the false riches of the East to the proverbial luxuries of Capua. Perhaps nowhere in Portuguese literature of the sixteenth century was the double standard more apparent than in the very epic of Portuguese expansion—the *Lusíadas* (1572) of Luís Vaz de Camões—which expressed the doubts and ideological contradictions of an entire era.

³⁶ "Y es cosa de ver, los historiadores portugueses cuánto encarecen por ilustres estas tan nefandas hazañas, ofreciéndolas todas por grandes sacrificios a Dios." Bartolomé de las Casas, *Historia de las Indias*, ed. Augustín Millares Carlo with an introductory study by Lewis Hanke, 3 vols. (Mexico, 1951), bk. 1, chap. 23. On Las Casas' debt to Zurara and Barros, see Zurara, *Chronique de Guinée*, ed. Bourdon, 14, 16–18.

THE DEVELOPMENT OF AN OFFICIAL IDEOLOGY to justify Portuguese commercial and military offensives in Africa and Asia, together with the self-doubts felt by those participating in or reporting on such events, influenced the manner in which Portuguese approached the issue of slavery from sub-Saharan regions and their attitudes toward blacks. The Portuguese had become accustomed to Moorish slaves in agriculture and in the cities. The designations "Moor" and "slave" had been interchangeable,³⁷ but by the fourteenth century Moors had been physically and culturally assimilated into the Portuguese way of life or had established themselves in Moorish quarters (*mourarias*). Familiarity with slavery as an institution and the presence of blacks on the peninsula failed to prepare the Portuguese populace for the harsh realities of the slave trade. Zurara, who became chief chronicler of the realm in 1454, has provided an eyewitness account of the unloading of the first slave cargo on a fateful August morning in 1444:

But what heart could be so hard as not to be pierced with piteous feeling to see that company? For some kept their heads low and their faces bathed in tears, looking one upon another; others stood groaning very dolourously, looking up to the height of heaven, fixing their eyes upon it, crying out loudly, as if asking help of the Father of Nature; others struck their faces with the palms of their hands, throwing themselves at full length upon the ground; others made their lamentations in the manner of a dirge, after the custom of their country. And though we could not understand the words of their language, the sound of it right well accorded with the measure of their sadness. But to increase their sufferings still more, there now arrived those who had charge of the division of the captives, and who began to separate one from another, in order to make an equal partition of the fifts; and then was it needful to part fathers from sons, husbands from wives, brothers from brothers. No respect was shewn either to friends or relations, but each fell where his lot took him. . . . And who could finish that partition without very great toil? For as often as they had placed them in one part the sons, seeing their fathers in another, rose with great energy and rushed over to them; the mothers clasped their other children in their arms, and threw themselves flat on the ground with them, receiving blows with little pity for their own flesh, if only they might not be torn from them.³⁸

Even Zurara was moved lachrymously to ask for divine forgiveness for the actions of his fellow countrymen. Despite differences of race, language, and religion separating the chronicler from the slaves, Zurara felt empathy for their plight. He observed, "For it is not their religion but their humanity that maketh mine to weep in pity for their sufferings . . . remembering that they too are of the generation of the sons of Adam." Zurara's feelings of revulsion were shared by other onlookers. Townsfolk from Lagos and peasants from the surrounding countryside had congregated to witness the spectacle. An oblique reference by Zurara suggests that the onlookers, despite the presence of Dom Henrique mounted on a horse, were so overcome by loathing that they intervened to impede the distribution of slaves. If Zurara the humanist felt

³⁷ Eugénio Lopez-Aydillo, "Los Cancioneiros Gallego-Portugueses como fuentes históricas," *Revue historique*, 57 (1923): 506–10.

³⁸ I have followed the English translation of Beazley and Prestage, *Chronicle of the Discovery and Conquest of Guinea*, 1: chap. 25. See Zurara, *Chronique de Guinée*, ed. Bourdon, 7, n. 6.

remorse, Zurara the chronicler was quick to note that the prince speedily renounced his share of slaves: "his chief riches lay in the accomplishment of his purpose; for he reflected with great pleasure upon the salvation of those souls that before were lost." Zurara emphasized benefits accruing to such slaves. Males were taught trades and skills. Some were manumitted and married Portuguese women. Females were adopted, inherited properties from their benefactors, and made good marriages. Provided that they accepted Christianity, according to Zurara, such slaves were well treated. For his lack of outrage at such inhumanity Zurara the historian earned the opprobrium of Las Casas, who described the Portuguese chronicler as "appearing to be possessed of little less insensitivity than the infante."³⁹

The first cargo of black slaves appalled and astounded Zurara and his contemporaries, but by the sixteenth century trade with West Africa and the presence of black slaves in Portugal had become so general that neither evoked popular unrest or indignation. But the royal stamp of approval and papal sanction for the trade did not still all doubts. Barros was humanist enough to experience qualms over slavery. Basing his history of the West African phase largely on Zurara's *Chronicle*, Barros tried to avoid the topic entirely; when avoidance was impossible, he resorted to circumlocutions or changes of emphasis. For example, according to Zurara, once Antão Gonçalves had achieved the commercial objectives of his voyage, he called his men around him and suggested, "How fair a thing it would be if we, who have come to this land for a cargo of such petty merchandise, were to meet with the good luck to bring the first captives before the face of our prince." For his part, Barros emphasized that Gonçalves wanted to take back to Dom Henrique a person versed in local languages ("algũa língua desta terra"), who would serve as an invaluable intermediary in future conversions to Christianity as well as in trading negotiations. Barros made no allusion in the first *Década* to the arrival and distribution of slaves at Lagos in 1444, referring merely to the honors and knighthood conferred by Dom Henrique on Lançarote.⁴⁰

Barros' effort to reconcile slavery with an epic history of great deeds is illustrated by his discussion of the motives for constructing the fortress at Elmina on the Gold Coast (present-day Ghana) in 1482. Its location made this Portuguese enclave ideal for the profitable export of gold and slaves. Barros, however, chose to portray the episode as part of a great plan for converting the natives. King Dom João II (1481-95) sought a site for establishing the "cornerstone of the Catholic Church in the East" ("primeira pedra da Igreja oriental"). Choice of the Gold Coast had been based on the argument that the

³⁹ "Gómez Eanes, portugués historiador, el cual parece tener poca menos insensibilidad que el infante . . ." Las Casas, *Historia de las Indias*, 1: bk. 1, chap. 24. And see Zurara, *Crônica dos feitos de Guiné*, chap. 26.

⁴⁰ Zurara, *Chronicle of the Discovery and Conquest of Guinea*, ed. Beazley and Prestage, 1: chap. 12; João de Barros, *Ásia: Dos feitos que os portugueses fizeram no descobrimento e conquista dos mares e terras do Oriente*, 6th ed., with notes by H. Cidade, 4 vols. (Lisbon, 1945-46), 1: chaps. 6, 8. On Barros' divided loyalties, see Saraiva, *História da cultura em Portugal*, 3: 277-342.

blacks of this region appeared more developed—from the Portuguese perspective—than their counterparts elsewhere in West Africa. Their readiness for trade with the Portuguese and their skills at establishing rudimentary lines of communication augured well, Barros maintained, for their acceptance of Catholicism. Material gain was held out as the bait that, if taken by the local peoples, would lead to their infinitely more valuable spiritual reward of conversion to Catholicism. He was careful to emphasize that proselytization of the heathen was the king's prime concern.⁴¹

Barros adopted two stratagems for expressing his own views, while in no way appearing disloyal to king or country. First, he placed all criticism of Portuguese expansion in the mouths of non-Europeans and non-Christians. The reply given by King Caramansa to Captain Diogo de Azambuja, who had requested permission to start construction at Elmina, falls into such a category. Barros commented on the intelligence and good judgment of the king, "albeit a barbarian." After having heard out Azambuja, the king noted the contrast between Azambuja's men and the unkempt appearance of sailors from earlier Portuguese vessels. Sick and badly clothed, the latter had been happy to accept anything in exchange for the baubles and trinkets they had to offer. Earlier voyages, the king remarked, had been moved by the desire for personal profit and a safe and speedy return to Portugal, rather than by any intent to remain in the tropics. Barros implied that Caramansa clearly saw behind the lofty words and empty promises of the Portuguese captain. His fear of hostility between the two peoples soon proved to be well founded. In his discussion of slavery, Barros wisely refrained from passing judgment on the peculiar profit motives of those Portuguese who traded merchandise for souls. If the motivation was profit, the court historian blandly declared, it was limited to the lower orders.⁴²

Second, he passed judgment on a situation or set of circumstances in a non-European context, although of no less pressing concern to Europeans. The institution of slavery is a case in point. Barros must have been aware of Asiatic slavery—the water carriers of Goa and forced prostitution of female slaves. He commented on the unusual position of slaves in Ormuz. Not only were they employed by the king as henchmen to terrify his subjects, but they occupied responsible administrative posts which the king was reluctant to entrust to local leaders for fear of treachery. That such slaves frequently killed their masters led Barros to reflect that "the human spirit does not submit to bondage willingly; everywhere in the world someone can always be found who willingly takes up arms in defense of freedom." His audience could not have missed the relevance of this comment to Portuguese circumstances, but no-

⁴¹ "Porque com esta isca de bens temporais que sempre ali haviam de achar, recebessem os da Fé, mediante a doutrina dos nossos, o qual efeito era o seu principal intento," Barros, *Ásia*, 1: bk. 3, chap. 1. On the fort, see Blake, *Europeans in West Africa, 1450–1560*, 1: 40–57; and A. W. Lawrence, *Trade Castles and Ports of West Africa* (Stanford, 1964), 103–15.

⁴² Barros, *Ásia*, 1: bk. 3, chap. 2. For a contemporaneous account, see Rui de Pina (ca. 1440–1514/23), *Crónica de El-Rei Dom João II*, ed. Alberto Martins de Carvalho (Coimbra, 1950), chap. 2; de Pina referred to the Portuguese in West Africa as being "scant in number, dirty, and of base birth."

where does Barros openly criticize Portuguese enslavement of blacks in West Africa. On one of the rare occasions when he interposed a personal commentary on blacks—praising their bravery and soldierly potential—he limited it to the narrow context of military conquest. Nowhere did he acknowledge broader humanitarian concerns or the growing importance of blacks in the economy of Portugal and its seaborne empire.⁴³

Writing in 1539, Barros was perceptive enough to recognize the commercial importance of the slave trade *per se* to Portugal. That a growing plantation economy had become dependent on this source of labor was obvious. Barros was historian enough to recognize the historical momentum—the landmarks had been the establishment of the trading post at Arguin, construction of the Elmina fortress, expansion of the sugar industry in Madeira, and the success of donatories in Brazil who had devoted limited financial and human resources to the cultivation of sugar in Pernambuco and São Vicente—which would lead inexorably to Portuguese introduction of plantation slavery into the Americas. Slaves of African origin may have been in Brazil as early as 1532. But Barros, if he foresaw this development, ignored it. He defended an official policy which was more medieval than Renaissance in outlook and which provided ideological justification for a sometimes distasteful economic and human reality.⁴⁴

The debate in Portugal over black slavery and the slave trade never equalled that in Spain over the enslavement of Amerindians. Apparently, no Portuguese theological or legal treatise appeared on this subject before the eighteenth century, but it was a recurring theme in Portuguese letters from the fifteenth to the nineteenth centuries. Theological and moral doctrines were tempered by commercial and political considerations. Although Christianity had taught that slaves should be treated humanely and that manumission of a slave was a pious act, the Catholic Church never condemned slavery as an institution. St. Augustine had advanced the moral and religious premise that slavery was a form of divine punishment for man's original sin. Absolute natural right had been destroyed by original sin, giving rise to the control exercised by man over man. For his part Thomas Aquinas believed that the weak could derive benefit from dominion by the strong, and his views are central to an understanding of attitudes toward slavery in Portugal and its empire. His *De Regimine principum* (1266) followed the Aristotelian doctrine of natural law—that is, that one part of mankind had been set aside by nature to be slaves in the service of masters born to a life free of such drudgery. In this context "slaves" referred to persons who, through weakness of physical or mental faculties or lack of willpower, were incapable of exercising moral choices on their own initiative. There were free men who were slaves by nature, and *vice versa*. Such a "slave" depended on a master to exercise his

⁴³ "... porque o ânimo humano sofre mal sujeição; e por causa desta liberdade não há parte no Mundo, onde se não ache mão armada pola defender"; Barros, *Asia*, 2: bk. 2, chap. 2. Boxer, *The Portuguese Seaborne Empire, 1415-1625*, 301-02.

⁴⁴ In 1552 Padre Antônio Pires referred to the "grande escravaria assim de Guiné como da terra"; (Pernambuco) *Cartas Jesuíticas*, vol. 2: *Cartas Avulsas, 1550-1568* (Rio de Janeiro, 1931), 123, n. 67.

options for him, and Aquinas believed that slavery performed a valuable service for slave and master alike.⁴⁵

The Christian doctrine that all men are created equal was accepted in so far as it applied to the spiritual sphere and the equality of the soul.⁴⁶ But it was not considered relevant to the more physical aspects of human existence and to relations between man and society. The primacy of society over the individual and the necessity of slavery as a social and economic institution were central to a line of thought that persisted in Portugal from the fifteenth to the nineteenth centuries. Historical antecedents for these views were not lacking. In his *Politics* Aristotle had observed that man is “by nature a political animal”—that he belongs to a *polis* and that a slave was no more than “a living instrument.” Advocates of Roman law could cite Justinian’s concise opinion that “slavery is an institution of the *jus gentium* [law of nations], whereby a man is, contrary to nature, subjected to the ownership of another.” Accordingly, the slave was a marketable item to be sold, traded, and purchased like any other commodity. The status of slave was hereditary but not irrevocably so, provided that the possibility of manumission existed. Toward the end of the colonial period in Brazil, this view continued to find expression in the works of Bishop Azeredo Coutinho of Pernambuco, whose *Analysis of the Justice of the Slave Trade on the Mina Coast* (1798) was written specifically to refute the claim that the slave trade to Portuguese America was unjust, at variance with natural law, or offensive to social conventions. While he recognized the Christian tenet that all men had been created equal, Azeredo Coutinho emphasized socioeconomic needs for slavery and observed that “nature created men for society . . . thus the rights of the individual were formulated at a later time than the law of society.”⁴⁷

In the Iberian peninsula both the principles justifying enslavement of blacks

⁴⁵ D. J. O’Connor, *Aquinas and Natural Law* (London, 1967); H. L. A. Hart, *The Concept of Law* (Oxford, 1961), 181–98; Mavis Campbell, “Aristotle and Black Slavery: A Study in Racial Prejudice,” *Race*, 15 (1974): 283–301; and Verlinden, “L’esclavage dans le monde ibérique médiéval,” 300–13. For sixteenth- and seventeenth-century Spanish treatises on “natural” and “legal” slavery, see Lewis Hanke’s *Cuerpo de documentos del siglo XVI sobre los derechos de España en las Indias y las Filipinas* (Mexico, 1943), and the bibliography to his *Estudios sobre Fray Bartolomé de Las Casas y sobre la lucha por la justicia en la conquista española de América* (Caracas, 1968), 131–89. Also see Silvio A. Zavala, *Servidumbre natural y libertad cristiana según los tratadistas españoles de los siglos XVI y XVII* (Buenos Aires, 1944). Zavala has published numerous studies on the legal position of Amerindians in Spanish America: *Los esclavos indios en Nueva España* (Mexico, 1968); *Las instituciones jurídicas en la conquista de América* (Mexico, 1971); *La encomienda indiana* (2d ed.; Mexico, 1973); and *The Defence of Human Rights in Latin America, Sixteenth to Eighteenth Centuries* (Paris, 1964).

⁴⁶ Zurara, a strong advocate of qualitative differences between men and the moral superiority of the noble and his pre-eminence in the social order, went against Christian tenets in proposing that such distinctions were no less present in the afterlife and that rewards and punishments were dispensed accordingly. Saraiva, *História da cultura em Portugal*, 1: 565–66.

⁴⁷ For a detailed study of Azeredo Coutinho’s attitudes toward slavery, as described in his *Análise sobre a justiça do comércio do resgate dos escravos da Costa d’Africa*, see Sônia Aparecida Siqueira, “A escravidão negra no pensamento do Bispo Azeredo Coutinho,” *Revista de história*, 27 (1963): 349–65, and 28 (1964): 141–98. The Academia das Ciências of Lisbon, considering his views on slavery too retrograde, refused to publish Azeredo Coutinho’s work. Also see Manoel Cardozo, “Azeredo Coutinho and the Intellectual Ferment of His Times,” in H. H. Keith and S. F. Edwards, eds., *Conflict and Continuity in Brazilian Society* (Columbia, S. C., 1969), 72–103; and E. Bradford Burns, “The Role of Azeredo Coutinho in the Enlightenment of Brazil,” *Hispanic American Historical Review*, 44 (1964): 145–60. Four of Azeredo Coutinho’s major essays have been edited, with an excellent introduction, by Sérgio Buarque de Holanda under the title *Obras economicas de J. J. da Cunha de Azeredo Coutinho* (São Paulo, 1966).

and the institution of slavery were subjects of debate. Lewis Hanke has described the manner in which Spanish experiences and experiments with Amerindian populations stimulated controversies in Spain, the most famous of which was the debate in 1550 between Las Casas and Juan Gines de Sepúlveda held in Valladolid. The prominence of this polemic and what Hanke refers to as the “dawn of conscience in America” have drawn attention away from the theological examination of the nature of the black slave from West Africa in the sixteenth century.⁴⁸

The Spanish Dominican friar Tomás de Mercado had witnessed the sale of black slaves in his native Seville. He criticized the manner in which the Portuguese conducted the trade and the cruelty meted out to slaves, for he found it difficult to reconcile reports of mass slave baptisms at embarkation on the West African coast with the indignities to which these new Christians were subjected on arrival in Spain. Mercado had lived in Mexico and had described the exploitation of man by man in his *Tratos y contratos de mercaderes y tratantes en ellas*, published at Salamanca in 1569. Whereas Mercado had emphasized physical hardships forced on black slaves, Friar Bartolomé Frias de Albornoz turned his critical gaze on the immorality of the slave trade. The first professor of law at the newly founded University of Mexico (1553), Albornoz characterized Portuguese activities on the Guinea coast in his *Arte de los contratos* as no better than common plunder, manifestly devoid of any criteria which might earn the designation “just war.” Mercado conceded that the reduction of blacks to slavery could be justified under certain conditions, but as a jurist Albornoz argued that the presumption of freedom was so strong that it outweighed mitigating circumstances. Albornoz was no less uncompromising in his condemnation of the sophistic argument that the salvation of souls counterbalanced the loss of liberty. By virtue of forty years’ residence in Portugal, Luís de Molina (1536–1600), the Spanish Jesuit theologian, was better placed than his contemporaries to witness the human suffering the slave trade imposed. Molina condemned the trade as practiced by the Portuguese in his *De justitia et jure*, the publication of which was begun in Cuenca in 1593 and was concluded posthumously. No participant emerged unscathed from his attack. He chastized the bishop and clergy of the Cape Verdes for their apathy and their failure to raise moral objections to the trade; Molina claimed they acquiesced by their very indifference. The Jesuit recalled that Charles V and Philip II of Spain had invited theological debates on the position of the Amerindian, and he deplored the failure of the kings of Portugal to stimulate similar discussions of the West African slave trade, which Molina considered wholly unjustified on moral and theological grounds. Its practitioners were, he declared, guilty of mortal sin.

Spanish thinkers of the sixteenth century—Mercado, Albornoz, Molina,

⁴⁸ Lewis Hanke, “The Dawn of Conscience in America: Spanish Experiments and Experiences with Indians in the New World,” *Proceedings of the American Philosophical Society*, 107 (1963): 83–92. Also see his *The First Social Experiments in America* (Cambridge, 1935) and *All Mankind Is One: A Study of the Disputation between Bartolomé de las Casas and Juan Gines de Sepúlveda in 1550 on the Intellectual and Religious Capacity of the American Indians* (De Kalb, Ill., 1974).

and others—accepted the institution of black slavery in principle. That the trade was predominantly in Portuguese hands provided Spanish critics with ready ammunition for nationalistic feeling and led some to adopt a “holier-than-thou” attitude. Others were chameleons, changing sides, or were ambivalent on the issue of black slavery. Las Casas, the indefatigable defender of the liberty of the Amerindian in Spanish America, had initially advocated the enslavement of blacks. Exposure to the harsh realities of the trade changed his views and led him roundly to condemn black slavery. Public officials, forced to reconcile dictates of conscience with administrative expediency, on occasion took refuge behind a double standard. The distinguished jurist and writer, Juan Solórzano Pereira (1575–1653/4), was a case in point. A loyal royal official, his experience in Peru as *oidor* of Lima from 1609 to 1627 almost dictated his ambivalence in defining those circumstances under which forced Indian labor could be justified. He concluded that employing Amerindians was permissible in public works, but not in the mines of Potosí. In the first decades of the seventeenth century careful reading of civil and canon law forced the Spanish Jesuit Alonso de Sandoval to acknowledge the validity of black slavery and the freedom of birth of the Amerindian. Born in Seville in 1576 and educated by the Society of Jesus in Lima, Sandoval was ordained at the turn of the century. Central to his mission in Cartagena de Indias was the awesome task of ameliorating the appalling physical and spiritual welfare of slaves already residing in and around Cartagena as well as of new arrivals from West African ports. Some fifteen years of experience coupled with exhaustive research into the baptismal credentials of black slaves provided the basis for his treatise, *De instauranda Aethiopum salute*, printed in Seville in 1627. Not as well known as it should be to modern scholars, Sandoval’s work provides a compelling indictment of the slave trade and is a mine of social and anthropological information. But his clarion call to his Jesuit colleagues fell on deaf ears. Portuguese Jesuits in West Africa and Brazil pointed to the stamp of legality granted to the trade by the Tribunal of Conscience (*Mesa da Consciência*) in Lisbon and, disregarding Sandoval’s exhortations as needless meddling, continued to condone and even to participate in the slave trade.⁴⁹

In Portugal itself some skeptics questioned the validity of arguments justifying an “ideology of expansion” in general and the moral and theological implications of the slave trade in particular. Doubts about slavery united such otherwise disparate personalities as Fernão de Oliveira (1507–81) and Amador Arrais (1530–1600). Oliveira had left the Dominicans, was hounded by the

⁴⁹ For an admirable summary of Spanish reaction, see Domínguez Ortiz, “La esclavitud en Castilla,” 406–18. On Las Casas, see Zavala, “¿Las Casas, esclavista?” *Cuadernos Americanos*, año 3, 2 (1944): 149–54; Fernando Ortiz, “La ‘leyenda negra’ contra Fray Bartolomé,” *Cuadernos Americanos*, año 11, 5 (1952): 146–84; and Marcel Bataillon, “Le ‘clérigo Casas’ ci-devant colon, réformateur de la colonisation,” *Bulletin Hispanique*, 54 (1952): esp. 366–69. For Sandoval’s views in particular, see Charles R. Boxer, *Salvador de Sá and the Struggle for Brazil and Angola, 1602–1686* (London, 1952), 237–40; Vincent P. Franklin, “Alonso de Sandoval and the Jesuit Conception of the Negro,” *Journal of Negro History*, 58 (1973): 349–60; and Norman Meiklejohn, “The Observance of Negro Slave Legislation in Colonial Nueva Granada” (Ph.D. dissertation, Columbia University, 1968), of which an extract was published in Lewis Hanke, ed., *History of Latin American Civilization: Sources and Interpretations*, 2 (2d ed.; Boston, 1973): 394–402.

Spanish Inquisition, and wrote an authoritative study on naval construction. Arrais, a barefooted Carmelite, devoted much energy to the ransom of Christians captured at the ill-fated battle of Alcazárquivir (August 4, 1578) and to the study of grammar. In their writings both men echoed earlier discontent at attempts made by the Portuguese crown, with papal sanction, to appease opponents of overseas expansion. They were especially incensed by the manner in which successive Portuguese kings had resorted to religious and moral arguments to justify blatantly self-serving political and economic ends. In the first decade of the seventeenth century a Portuguese Jesuit submitted a memorial (which was not printed) to the crown on the enormities of the slave trade as practiced in the Portuguese conquests. Basing his discussion on points made by Mercado, the anonymous author argued for the extinction, or at least major reform, of the trade.⁵⁰

In Brazil debate was more pragmatic. Black slavery was accepted, but certain aspects of that “peculiar institution” raised doubts in the minds of missionaries and colonists alike. One particular concern, later to be shared by English colonists in North America, involved the freedom which might ensue for slaves who converted to Christianity. Concubinage between white males and black female slaves made freedom a key issue in Portuguese America in the sixteenth century. White colonists were allegedly reluctant to sanctify such unions for fear that marriage to a slave would automatically result in freedom for both the wife and her offspring. The Jesuit Manoel da Nóbrega (1517–70), who was born in Portugal and died in Rio de Janeiro, sought without great success to assuage such doubts. Another Jesuit, Fernão Cardim, who accompanied Visitor Cristóvão de Gouveia to Brazil, noted that colonists in Bahia in 1583 still needed reassurance on this issue. Jesuits also attempted to promote marriages between blacks in which both parties were slaves. In 1551 still another Jesuit, António Pires, wrote from Pernambuco that the Order had had some success in persuading slaves to marry “at the church doors”; but there would be more such marriages, he claimed, if only the owners could be persuaded that wedlock would not result in manumission.⁵¹

Not until the eighteenth century did the climate of opinion in the colony become more critical not only of the cruelty inherent in slavery but also of the institution itself. Interestingly enough, such criticism was most vocally expressed and best articulated in the works of Europeans who had resided in Brazil for considerable periods. The Rimini-born Jesuit Jorge Benci, who taught scholastic theology in Salvador from 1684 to 1687, described the maltreatment of black slaves in his *Economia Christã dos senhores no govêrno dos escravos*, published in Rome in 1705. His fellow countryman and Jesuit, Gio-

⁵⁰ Godinho, *Os descobrimentos e a economia mundial*, 557–59; and Boxer, *Salvador de Sâ and the Struggle for Brazil and Angola*, 237.

⁵¹ Fernão Cardim, *Tratados da terra e gente do Brasil*, Introduction by Batista Caetano, Capistrano de Abreu, and Rodolpho Garcia (Rio de Janeiro, 1925), 300. For Pires’ letter of August 2, 1551, see *Cartas Jesuíticas*, 81. Fears of the role of conversion were equally present in the English colonies in North America; see Winthrop D. Jordan, *White over Black: American Attitudes toward the Negro, 1550–1812* (Chapel Hill, 1968), 92–93.

vanni Antonio Andreoni, also exposed the horrors of slavery in his *Cultura e opulência do Brasil por suas drogas e minas*, which was published in Lisbon in 1711, only to be suppressed. The excessive punishment of slaves concerned Lisbon-born Dr. Manuel Ribeiro Rocha, who practiced law in Bahia. His *The Ethiopian Ransomed, Indentured, Sustained, Corrected, Educated, and Liberated* (as the title reads in English translation), published in Lisbon in 1758, went beyond the works of his predecessors in listing abuses and openly advocated the replacement of black slavery in Brazil by indentured labor. An anonymous pamphlet published in the same city in 1764, *New and Curious Relation of a Grievance Redressed* (also as the title reads in translation), challenged the hallowed belief that the black was born to serve the white. Written in the form of a dialogue between a lawyer and a miner, the *New and Curious Relation* dramatically revealed the irreconcilable differences between the enlightened minority and the Portuguese majority on the issue of black slavery. But, despite the concern evidenced by such works, few authors treated an institution so central to the social and economic well-being of the colony. The protests of the few appeared in small editions which had limited circulation at best, and they were little read in either Portugal or Brazil.⁵²

PORTUGUESE ATTITUDES TOWARD BLACK SLAVERY, both in the mother country and later in the colony, were not limited to the realm of theological and moral debate. Much has been made of the centuries of contact between Portuguese, Moors, Jews, and peoples from the sub-Saharan regions of Africa. Scholars such as Frank Tannenbaum and Stanley Elkins have accepted at face value travel accounts of race relations in Brazil and have followed the lead of the Brazilian historian and sociologist Gilberto Freyre in attributing to the Portuguese an unusual capacity for assimilating and tolerating other races.⁵³ Even the early evidence from the mother country points elsewhere. Portuguese writers of the fifteenth and sixteenth centuries were less than complimentary about the physical appearance and moral state of blacks from sub-Saharan regions.

Zurara, chronicler of early encounters between Portuguese and blacks in West Africa, adopted a yardstick based on varying degrees of pigmentation to measure the physical appeal or revulsion of whites toward the slaves landed at Lagos in 1444: "And these, placed all together in that field, were a marvelous sight; for amongst them were some white enough, fair to look upon, and well proportioned; others were less white like mulattoes; others again were as

⁵² Charles R. Boxer, *Race Relations in the Portuguese Colonial Empire, 1415-1825* (Oxford, 1963), 103-13. Also see his annotated English translation of the *Nova e Curiosa Relação*: "Negro Slavery in Brazil: A Portuguese Pamphlet (1764)," *Race*, 5 (1964): 38-47.

⁵³ Freyre, *Brazil: An Interpretation* (New York, 1945), 18-22, and *The Masters and the Slaves: A Study in the Development of Brazilian Civilization*, trans. Samuel Putnam (2d Eng. ed., rev.; New York, 1966), Preface to the first Eng. ed. (dated 1945), xiii, and 2-17. And see Frank Tannenbaum, *Slave and Citizen: The Negro in the Americas* (New York, 1946); Stanley M. Elkins, *Slavery: A Problem in American Institutional and Intellectual Life* (Chicago, 1959); and Donald Gray Eder, "Time Under the Southern Cross: The Tannenbaum Thesis Reappraised," *Agricultural History*, 50 (1976): 600-14.

black as Ethiops, and so ugly, both in features and in body, as almost to appear (to those who saw them) the images of a lower hemisphere."⁵⁴ In four of his plays Gil Vicente included black characters, generally in burlesque roles. The butt of the heavy-handed and caustic Vicentine humor lay precisely in those physical attributes and defective speech patterns allegedly found among blacks in Portugal. In the *Forge of Love* (*Frágoa d'amor*, 1524) Cupid, the master smith, was assisted by Jupiter and Mercury. Persons dissatisfied with their physical appearance were given the opportunity to change their bodily features. The first applicant was a black who wished to "become as white as a hen's egg," with a well-shaped nose and thinner lips. Although the transformation was accomplished, the former black was dissatisfied because he had retained his "deuced speech of Guinea" and still did not speak fluent Portuguese. In the allegorical *Boat of Loves* (*Nau d'amores*, 1527) Vicente introduced a son of the king of Benin. Of his father's forty sons he believed that he was the noblest; yet this availed him nought in his suit for the hand of a Portuguese maiden, who rejected his overtures with such epithets as "cur" and "cabraão," the latter translating literally as "big goat"—intended here to carry a derogatory racial connotation. In his use of "black Portuguese" Vicente continued a tradition already evident in the *Cancioneiro Geral* (1516); he bequeathed to his successors in popular theater the character of the black man as an object of ridicule.⁵⁵

Writers were not content merely to list the physical defects (as they saw them) of blacks: authors excoriated not only the slaves' blackness, but their supposed savagery, their "brutish ways," their libidinousness, their moral degeneracy, and their unorthodox religious practices. Zurara referred to their "great bestiality" and their disordered—by Portuguese standards—physical and moral conduct. He lamented that blacks were both ignorant of the need to provide food and shelter on a regular basis and shameless enough to go naked. Their apparent failure to distinguish clearly between moral right and wrong was no less disturbing to the chronicler than the apparent absence of a "work ethic."⁵⁶ Zurara and some contemporaries thought the Portuguese were charged with a dual responsibility: as Catholics their duty lay in bringing unbelievers into the Christian fold; as agents of civilization their moral obligation lay in civilizing blacks who would otherwise remain in "bestial sloth." Writing in 1523, the Italian philosopher Giulio Landi noted that, in assessing the merits of black slaves, the Portuguese adopted three criteria: religious orthodoxy (or lack thereof); circumstance of birth (offspring of slave parentage); and color, whether black or mulatto. Although the great Jesuit missionary and preacher António Vieira (1608–97), who had a mulatto

⁵⁴ Zurara, *Chronicle of the Discovery and Conquest of Guinea*, ed. Beazley and Prestage, 1: chap. 25.

⁵⁵ Paul Teyssier, *La langue de Gil Vicente* (Paris, 1959), 227–50. For chronology, see Anselmo Braamcamp Freire, "Trovador, mestre da balança": *Vida e obras de Gil Vicente* (2d ed.; Lisbon, 1944). Also see Edmund de Chasca, "The Phonology of the Speech of the Negroes in Early Spanish Drama," *Hispanic Review*, 14 (1946): 322–39. For a modern counterpart, see Richard A. Preto-Rodas, *Négritude as a Theme in the Poetry of the Portuguese-Speaking World* (Gainesville, Fla., 1970).

⁵⁶ Zurara, *Crónica dos feitos de Guiné*, chaps. 25, 35, 76.

ancestor on his father's side, insisted that religious orthodoxy (Catholicism) rather than racial purity (whiteness) was the distinguishing trait of a cultured person, this position did not prevent him from advocating increased slave imports from Africa into Brazil to guarantee the continued freedom of Amerindians. Medieval theory, according to which blacks were descended from the accursed Ham and thereby subject to eternal slavery, melded with the tradition that blacks were descendants of Cain, who had been cursed by God. Though with many variations, this same basic theme can be found in the literature on slavery from Zurara in the mid-fifteenth century to the Jesuit Jorge Benci and the anonymous author of the *New and Curious Relation* in the eighteenth century who associated blacks with bondage.⁵⁷ That such attitudes were not limited to Portuguese or to Catholics has been dramatically illustrated by Winthrop Jordan in his detailed analysis of the various English postures in the American colonies.⁵⁸

These values and attitudes were reflected in legislation which, although drawn up in Lisbon for Portugal, was equally binding on colonies and outposts of empire where whites, for the most part, were in the minority. Laws and decrees attempted to regulate relationships between white Catholic Portuguese and a wide variety of other races, religions, national loyalties, and value systems that rarely coincided with those of the Portuguese. The crown was slow to enact legislation protecting blacks, Asiatics, and Amerindians against abuse. In 1570 a law was promulgated forbidding Portuguese enslavement of the Japanese, and a law of 1624 extended the prohibition to the Chinese. Such legislation failed to prevent the Portuguese in Goa, Malacca, and other outposts in Asia from taking slaves. Often the Portuguese conformed to local practices or acted with the knowledge of local authorities. When in 1587 the vice-provincial of the Society of Jesus in Japan, Gaspar Coelho, was asked by Toyotomi Hideyoshi why the Portuguese bought and exported Japanese as slaves, the Jesuit sharply reminded Hideyoshi that the sellers were Japanese and that Hideyoshi himself could stop the traffic by decree. Regarding the Amerindians, King Dom Sebastião (1568–78) ruled on March 20, 1570 that they should not be made slaves by the Portuguese colonists in Brazil, except in certain circumstances which included “just war.” Variations on this law and measures clarifying conditions under which Amerindians could legitimately be employed were further laid out in legislation of the later sixteenth and seventeenth centuries—much of it during the period of “Philippine domination” (1580–1640). Legislation proved no more effective in Portuguese America than in Portuguese Asia. Complaints that the Portuguese in Brazil enslaved Amerindians with virtual impunity were as rife as they were well founded throughout the colonial period.

It was left to the Marquis of Pombal, minister to King Dom José I (1750–

⁵⁷ Godinho, *Os descobrimentos e a economia mundial*, 561; and Charles R. Boxer, *A Great Luso-Brazilian Figure: Padre António Vieira, S. J., 1608–1697* (London, 1957), 22–23. On the Ham/Cain controversy, see Zurara, *Chronique de Guinée*, ed. Bourdon, chap. 16 and n. 5; and Boxer, “Negro Slavery,” 40–41.

⁵⁸ Jordan, *White over Black*, 3–43.

77), to take a firm stand on the emancipation of Amerindians and Asiatics. Decrees promulgated between 1755 and 1758 secured the freedom of Amerindians and attempted to provide opportunities for their assimilation into the society of Portuguese America. But the Portuguese did not enter into binding contracts or treaties with the Amerindians in Brazil. Indeed, such was the strength and warlike nature of the Amerindian population at the beginning of the nineteenth century that on May 3, 1808 Dom João VI declared war on the Botocudos of Minas Gerais and military expeditions were dispatched in Espírito Santo, Pôrto Seguro, Bahia, and São Paulo to “pacify” hostile Indians. A decree of 1758 formally abolished the enslavement of Chinese. An edict of April 2, 1761 ordered that all Asiatic subjects of the Portuguese crown who were baptized Christians should enjoy the same legal and social standing as white persons born in Portugal. Here Pombal followed the intent of the earlier laws of 1562 and 1572, which had been ignored by authorities in the “State of India” (*Estado da Índia*). In Goa concerted official efforts attempted to prevent the edict of 1761 from becoming widely known; it was not promulgated there until 1774.⁵⁹

Significantly, no parallel measures were proposed bearing on the social and economic status of the largest slave, nonwhite, and non-European population of any Portuguese territory—Brazil. An edict of September 19, 1761 ruled that, beginning six months from the date of publication, black slaves landing in Portugal would automatically be freedmen. In 1773 a royal decree emancipated black slaves in Portugal. But Brazil was not included. Nor was a corpus of legislation corresponding to the French *Code Noir* (1685), to the Spanish *Código Negro Carolino* (1785) or *Código Negro Español* (1789), or even to the slave codes of English colonies in North America ever formulated for Brazil. Only in 1888 was slavery abolished in Brazil, sixty-six years after the end of Portuguese domination. During its more than three hundred years as a Portuguese colony, Brazil had been governed by civil and criminal legislation codified during the reign of Dom Manuel I (1495–1521) and recodified in 1603. The *Ordenações Manuelinas* (1514, 1521) was a compendium of laws issued by Dom Manuel and his predecessors in years of social, political, and economic change and military strife during which Portugal had progressed from the consolidation of its national territorial boundaries to the establishment of a Portuguese presence in Africa, Asia, and the Americas.

Medieval attitudes and concepts, the code of chivalry, and respect for biblical authority, scholastic theories, and theological doctrines did not suddenly evaporate in an age of maritime exploration and discovery. Empirical knowledge and a confidence often associated with the so-called Renaissance were tempered by cynicism and self-doubts born of false hopes and illusions.

⁵⁹ For a discussion of the practical considerations leading Pombal to abolish slavery in Portugal, see Francisco C. Falcon and Fernando A. Novais, “A extinção da escravatura africana em Portugal no quadro da política econômica pombalina,” *Anais do VI Simpósio Nacional dos Professores Universitários de História* (São Paulo, 1973): 405–31. Also see Boxer, *Race Relations in the Portuguese Colonial Empire*, 69–75, 98–101, *Fidalgos in the Far East*, 225–41, and *The Christian Century in Japan, 1549–1650* (Berkeley and Los Angeles, 1967), 146–47.

It was perhaps inevitable at a time of national transition that attitudes should reflect personal dilemmas and ideological contradictions which verged occasionally on the paradoxical in the conflict between continuity and change. In four centuries Portugal progressed from a comparatively unimportant nation on the western rim of Christendom to a global seaborne empire, only to be eclipsed by the Dutch and English. Portuguese attitudes justifying black slavery, rooted in medieval views of man and society, survived in colonial Brazil and, after 1822, in an independent Brazil, despite their demise in Portugal itself. The fateful legacy endured in the New World long after it had been disavowed in the land that bequeathed it.

Indian Labor and New World Plantations: European Demands and Indian Responses in Northeastern Brazil

STUART B. SCHWARTZ

IN RECENT YEARS INTEREST in the early years of European conquest and colonization in the New World has undergone remarkable resurgence. No longer concerned solely or primarily with the effect of this process on subsequent national histories, scholars have increasingly turned their attention to the role that the New World colonies played in the creation of an Atlantic—even a world—economic system. This resurgence of interest has resulted in lengthy and often heated debates on the nature of colonization in the sixteenth and seventeenth centuries and on the dominant mode of production which prevailed in these colonies. Recently, Immanuel Wallerstein has argued that the Americas, as a peripheral zone of capitalist expansion, experienced certain forms of coerced labor such as chattel slavery and the *encomienda*, both of which were necessary forms of colonial exploitation that permitted the formation of a surplus sufficient to make such colonial ventures worthwhile.¹ A Brazilian scholar, Fernando Novais, has even suggested that European merchants seeking high returns on investments were responsible for imposition of the Atlantic slave trade and that the slave trade created African slavery in the

I would like to express my thanks to Richard Graham, David Sweet, Consuelo Pondé de Sena, Dauril Alden, and Stanley Engerman for helpful suggestions and criticisms. Part of the research for this article was done with the aid of grants from the American Council of Learned Societies (1974–75) and the University of Minnesota Graduate School (1972). The following abbreviations have been used: *ABNR* (*Anais da Biblioteca Nacional do Rio de Janeiro*); *AGS* (Arquivo General de Simancas); *AHU* (Arquivo Histórico Ultramarino, Lisbon); *ANTT* (Arquivo Nacional da Torre do Tombo, Lisbon); *BA* (Biblioteca da Ajuda, Lisbon); *BI* (Biblioteca do Palácio da Itamaraty, Rio de Janeiro); *BNL* (Biblioteca Nacional de Lisboa); *Cart. Jesuítas* (*Cartório dos Jesuítas*); *DH* (*Documentos Históricos da Biblioteca Nacional do Rio de Janeiro*, 120 vols. [1928–]); *DHA* (*Documentos para a História de Açúcar*, 3 vols. [Rio de Janeiro, 1954–63]); *HCJB* (Serafim Leite, ed., *História da Companhia de Jesus no Brasil*, 10 vols. [Lisbon, 1938–50]); and *MB* (Serafim Leite, ed., *Monumenta Brasiliæ*, 4 vols. [Rome, 1956–60]).

¹ Wallerstein, *The Modern World System* (New York, 1974). His bibliography provides an excellent introduction to the historical and theoretical literature. Wallerstein has recently anticipated my criticism and similar observations made by Domenico Sella by stating that “the alternatives available to each unit are constrained by the framework of the whole even while each actor opting for a given alternative in fact alters the framework of the whole.” Thus, the difference in our positions may be one of emphasis, although it is clear that, for him, the alternatives are still determined by the system and not by the actors. See his essay, “The Three Stages of African Involvement in the World Economy,” in Peter C. W. Gutkind and Immanuel Wallerstein, eds., *The Political Economy of Contemporary Africa* (London, 1976), 30. Also see Domenico Sella, “The World System and Its Dangers,” *Peasant Studies*, 6 (1977): 29–32. For an important set of essays on these problems, see Carlos Sempat Assadourian *et al.*, eds., *Modos de produção em América Latina*, vol. 40 of *Cuadernos de Pasado y Presente* (Buenos Aires, 1973).

New World, not the reverse.² Thus, the nature of the dominant mode of production and the creation and use of a labor force have become issues crucial to an understanding of the New World.

The broad sweep of these theses tends to leave the impression that the growth of capitalism was an inexorable process resulting from conscious decisions and choices made at the center of the world economy. This impression is Eurocentric, not in the old sense of cultural myopia but in the emphasis on Western desires and decisions without much regard for the objective cultural and physical realities in the "colonial" areas. Examining the nature of Indian slavery in the formative period of a plantation economy in northeastern Brazil can be a way of demonstrating how accurate the impression is, how local conditions and the specific cultures of non-Europeans shaped the formation of the various colonial regimes.

Throughout the Americas European powers attempted to make use of the American Indians as a source of labor. With a few major exceptions these attempts proved unsuccessful. Still, from the Carolinas to Santo Domingo and Brazil attempts to enslave native Americans preceded the period of African slavery.³ For plantation agriculture Indian slavery proved transitory, but in frontier regions like northern Mexico and the Amazon it lasted until the nineteenth century. The attempt to use Indians as a coerced labor force, in any case, cannot be simply dismissed as a "false start." The complex interplay of European and Indian perceptions and actions determined the ways in which Indians did—and did not—become integrated into the colonial regimes. In Brazil, Indian slavery had a short history in legal terms (roughly from 1500 to 1570), but various forms of coercion were used well after those dates to acquire indigenous laborers. Even after the large-scale introduction of African slaves, Indians continued to be a major source of labor. An examination of both the general outlines of the history of Indian labor in northeastern Brazil and the nature of life for Indians on the sugar plantations should help explain the role that Indians played in this plantation economy and the reasons that impelled the Portuguese to turn to Africans in the Atlantic slave trade.

A FULL ETHNOGRAPHY OF THE INDIGENOUS PEOPLES of Brazil on the eve of European colonization would be pointless here, but some aspects of the society and economy of the major groups encountered by the Portuguese on the Brazilian coast do help clarify the process of their absorption into a plantation economy. The most numerous and widely dispersed of the Indian peoples who came into contact with Europeans in the first two centuries of Brazilian colonial history spoke the Tupi-guaraní languages⁴ and controlled

² Fernando Novais, *Estrutura e dinâmica do antigo sistema colonial (Séculos XVI–XVIII)*, Caderno CEBRAP, no. 17 (São Paulo, 1974). Also see his "O Brasil nos quadros do antigo sistema colonial," in Manuel Nunes Dias *et al.*, eds., *Brasil em Perspectiva* (São Paulo, 1968), 53–71.

³ Winthrop Jordan, *White Over Black* (Chapel Hill, 1968), 89, provides a perceptive discussion of English attitudes toward Indians. Also see note 83, below.

⁴ See the survey presented in Estevão Pinto, *Os indígenas do Nordeste*, 1 (São Paulo, 1935): 168–246. Also see Carlos Ott, *Pre-história da Bahia* (Salvador, 1958), 11–33; Alfred Métraux, "The Tupinambá," in Julian Steward, ed., *Handbook of South American Indians*, 3 (Washington, 1948): 95–135; and Julio Cezar Melatti, *Índios do Brasil* (Brasília, 1960). For the best single sixteenth-century source on the indigenous peoples of

much of the littoral from Maranhão to Santa Catarina. Of these groups the best ethnographic information is available on the Tupinambá, who dominated the coast around the Bay of All Saints. They lived in villages of four to eight hundred individuals organized into large family units which shared some four to eight long houses. Patrilineal kinship was central to their societal organization, but divisions of sex and age also defined responsibility and privilege.⁵

The early colonists adopted the Tupinambá practice of calling all non-Tupi speakers by the general term "Tapuya"—speakers of twisted tongues. For many years anthropologists believed that these peoples belonged to the Gê linguistic family. Many did, but it is now clear that the peoples the Portuguese called Tapuya belonged to a number of linguistic and cultural groups.⁶ Since the Tupinambá occupied much of the coast, most of the other peoples had far less contact with the Portuguese in the sixteenth century. Their hunting and gathering economy tended to produce lower population densities, a simple material culture, and a nomadic existence. Migration, intertribal warfare, and Portuguese slaving did, however, bring these peoples within the European orbit. The Portuguese naturally considered the Tapuya particularly barbarous and irrational, in part because their very mobility made groups like the Aimoré effective military opponents.

In Tupinambá society, the acquisition of status, the choice of marriage partners, and progress through the ranks of age largely depended upon the manly activities: the capture of enemies in war and their eventual death as victims in a feast of ritual cannibalism.⁷ This need for captives necessitated constant warfare among the various Indian peoples; and ritual cannibalism, naturally abhorrent to the Portuguese, became a principal excuse for the enslavement of the Tupinambá and other peoples. Although warfare and ritual cannibalism perhaps provided the underpinnings of the Tupinambá's view of the universe and social organization, other features of Tupinambá culture are important for understanding Indian relations with the Portuguese. Unlike some of their neighbors, the Tupinambá practiced agriculture; it was well suited to their habitat and needs and it formed an important part of Tupinambá life.⁸ Essential to their subsistence was manioc, which they grew as their primary crop. The root flourished in a wide variety of soils, was resistant to the ravages of most insects, provided a high number of calories in relation to the area planted, and needed little care after planting. Made into a flour, manioc became the principal food of European as well as native Brazilians. And, along with maize, manioc was one of the primary Indian contributions to the world's diet.

Bahia, see Gabriel Soares de Sousa, *Tratado descritivo do Brasil em 1587* (4th ed.; São Paulo, 1971), 299–341.

⁵ Florestan Fernandes, *Organização social dos Tupinambá* (2d ed.; São Paulo, 1963), 149–309; and Métraux, "The Tupinambá," 119–26. Also see Egon Schaden, *Aspectos fundamentais da cultura guaraní* (São Paulo, 1962).

⁶ Ott, *Pre-história da Bahia*, 11–33; and Soares de Sousa, *Tratado descritivo do Brasil em 1587*, 78–80.

⁷ Florestan Fernandes, *A função social da guerra na sociedade Tupinambá* (2d ed.; São Paulo, 1970).

⁸ A. Métraux, *La religion des Tupinambá* (Paris, 1928), 170–71; and Fernandes, *Organização social dos Tupinambá*, 82–98. Métraux calls the Tupinambá an "agricultural people," but it is clear from his work and that of Fernandes that agriculture was not a major ceremonial force in Tupinambá society.

The Tupinambá employed a form of slash-and-burn agriculture (*coivara*) still in use in parts of Brazil today. The men undertook the heavy labor of opening a field by felling the large trees and then set fires to clear the underbrush; the ashes provided a natural fertilizer. To the women fell the tasks of planting, harvesting, and food preparation. Thus, routine farming was almost exclusively women's work. The men supplemented the Tupinambá diet with game and fish; but only in the heavy communal labor of clearing timber did the men enter the agricultural cycle.⁹ The Tupinambá economy was, therefore, basically one of subsistence with a communal or reciprocal attitude toward production. Land and sustenance were distributed according to lineage affiliations; every member of the group through familial ties was assured a minimum part of the whole. "These Indians maintain the time of the Apostles," one Jesuit commented. "They have nothing of their own; everything is common among them."¹⁰ Father Manoel da Nóbrega also suggested the Tupinambá "owned all things communally and that which one has must be divided with the rest, principally if it be foodstuffs." Another Jesuit, Juan de Azpilcueta Navarro, made a classic observation of the more nomadic Indians when he wrote that the Tapuyas of Porto Seguro were "very poor, owning nothing of their own privately; they eat in common that which each day they hunt and fish."¹¹

If the Indian economy was essentially communal and subsistent, it was autoconsumptive as well. Each village produced what it needed and depended very little on trade in foodstuffs.¹² It was, therefore, an economy based on production for use rather than for exchange, a system which provided a comfortable livelihood without concern for profit in the Western sense. There was no need to produce to the capacity of time or technology. In such an economy, the tempo of work and of production was intermittent and discontinuous;¹³ time was always left for leisure and other "nonproductive" activities like warfare and ceremony. The habitat of coastal Brazil facilitated such attitudes, since an adequate food supply could be obtained without extraordinary effort. This relatively secure food supply also made it quite easy for the Portuguese in Bahia to obtain manioc flour (*farinha*) and other food by trade in the period of early contact; and during the 1550s they purchased large

⁹ On women in agriculture, see the cross-cultural study by Michael Burton, Lilyan A. Brudner, and Douglas R. White, "A Model of the Sexual Division of Labor," *American Ethnologist*, 4 (1977): 227-50. Also see George P. Murdock, "Factors in the Division of Labor by Sex," *Ethnology*, 12 (1973): 203-25.

¹⁰ Martin da Rocha (September 1576), as quoted in *HC7B*, 2:90. Also see Ambrósio Fernandes Brandão, *Diálogos das Grandezas do Brasil*, ed. José Antônio Gonçalves de Mello (2d complete ed.; Recife, 1966), 199.

¹¹ Nóbrega, "Informação das terras do Brasil, 153; and *MB*, 2: 249 (1555). Observations like these are probably not quite true. Most Indian peoples recognized individual ownership of goods and objects of production (bows, axes, etc.) but collectively made use of goods of consumption. See Melatti, *Índios do Brasil*, 68-69. On the economy of a Gê group, see Roberto da Matta, "Notas sobre o contato e a extinção dos índios Gaviões do meio Rio Tocantins," *Revista do Museu Paulista*, new ser., 14 (1963): 192-93.

¹² Fernandes, *Organização social dos Tupinambá*, 84-85. Trade was not entirely lacking. For an important theoretical essay, see Claude Lévi-Strauss, "Guerra e comércio entre os índios de América do Sul," *Revista do Arquivo Municipal* (São Paulo), 87 (1942): 131-46.

¹³ These comments are based on Marshall Sahlins, *Stone Age Economics* (Chicago, 1972), 1-41. Although his analysis deals with Stone Age economics in general, it is directly applicable, contemporaneous observations indicate, to the Amerindian economies under consideration here.

amounts of farinha from villages along the coast.¹⁴ The Tupinambá, after satisfying their own needs, attached little importance to their surplus and willingly traded it for useful European goods on a limited basis. But, since the limits of the Tupinambá's willingness were finite, they proved to be an undependable source of food and, later, of labor.

A communal or reciprocal attitude toward production and consumption, the domestic mode of production, a society on which status was not derived from economic ability, and the subordination of the economy to other forms of social organization determined Indian responses to European demands. The divergent outlooks of Portuguese and Indians toward the nature and goals of labor and production lie beneath the change in Portuguese-Indian relations in the sixteenth century and help explain the subsequent history of Indian slavery.

Undependability, seeming prodigality, and lack of interest in profit, surplus, and savings grated upon European sensibility; and more than once such attitudes were offered as proof of Indian irrationality and thus evidence of their lack of "humanity." In 1610 Governor Diogo de Meneses wrote, "These Indians, Sir, are a very barbarous people who have no government and are unable to govern themselves; they are so lacking in this regard that even in their sustenance they will not save for tomorrow that which is in excess today. . . ." "Inconstant and changeable," Jesuit Simão de Vasconcellos called them. "That which they struggle to gain today with great labor and sweat tomorrow they disregard."¹⁵ Among the agricultural Tupinambá, the Portuguese were aghast at the "idleness" of the villages, where the men seemed to loll about smoking and preparing for battle.¹⁶ Portuguese attitudes toward the "barbarism" of the agricultural Amerindians were intensified in the face of tribes who lived only by hunting and gathering. It was the confrontation of two peoples whose economic systems and visions of life were worlds apart.

European perceptions and European demands had to be harmonized with the colony's need for food, labor, and defense. The earliest European activity in Brazil was the cutting and export of dyewood logs from the famous brazilwood trees. As logging depleted the supply along the coast, the Europeans increasingly turned to the Indians to provide the wood. Because communal labor, especially the felling of trees, was a characteristic masculine chore in Tupinambá society, this activity was easily integrated into the traditional patterns of indigenous life. Indians seemed quite willing to cut the trees and drag the heavy logs to the coastal region where they could be exchanged for trinkets and other trade goods. Between 1500 and 1535 the

¹⁴ Alexander Marchant, *From Barter to Slavery* (Baltimore, 1942), 87-95.

¹⁵ Diogo de Meneses to the crown, September 1, 1610, ANTT, Fragmentos, caixa 1, no. 6; and Simão de Vasconcellos, *Chronica da Companhia de Jesu do Estado do Brasil*, 3 vols. in 2 (Lisbon, 1865), 1: lxxxii.

¹⁶ "They are great friends of leisure (*folgar*)," Os Capítulos de Gabriel Soares de Sousa, *ABVR*, 62 (1940): 373. For an interesting comparison see Edmund Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), esp. chap. 3: "Idle Indian and Lazy Englishman"; and Richard R. Beeman, "Labor Forces and Race Relations: A Comparative View of the Colonization of Brazil and Virginia," *Political Science Quarterly*, 76 (1971): 609-36. Royal administrators in Spanish America used the "natural idleness" of Indians to justify forced labor. See Josefina Cintrón Tiryakian, "La imagen económica del indio," in *Actas del XLI Congreso Internacional de Americanistas*, 3 vols. to date (Mexico, 1976-), 2: 429-35.

Portuguese used barter as the principal means to obtain both brazilwood and, secondarily, manioc flour from the Indians. Barter was also an indirect method for obtaining labor. We do not know whether the Tupinambá and others traded logs, manioc flour, and labor individually or communally during this period, but records of purchases made in the 1540s indicate that the latter was true. If so, the barter system functioned within the parameters of community activity and was all the more adaptable to traditional patterns of life.

With the introduction of the donatary system in the 1530s, the situation in colonial Brazil began to change. The crown granted rights to Portuguese noblemen who, by settling colonists and establishing a secure economic basis, made new demands on the Indian inhabitants. The donataries and the new colonists continued to barter for the brazilwood, food, and even labor needed on a short-term basis for town building—all suitable to the traditions of intermittent effort, sexually defined roles, and communal advantage. But the sustained and continuous agricultural labor of the kind needed to raise and harvest the new crop of the settlement—sugar—could not be obtained by barter. In Bahia, Pernambuco, and elsewhere along the coast, the Portuguese increasingly turned to chattel slavery as a means of securing labor for the canefields and mills. They moved, as Alexander Marchant so ably put it, “from barter to slavery.”

In his important study, Marchant has argued that the barter system began to collapse because of a series of economic decisions made by the Portuguese and, to a lesser extent, by the Indians as well. First, there was a glut on the “trinket” market. As Indian demands shifted to more expensive ironware and firearms, the Portuguese costs of supply rose markedly. The increased number of colonists and the presence of royal brazilwood contractors, moreover, created competition for labor among the Portuguese and gave the Indians several markets for their goods and services. Faced with this disadvantageous situation, the Portuguese had to shift increasingly to slavery.

Marchant’s interpretation, while correct in its broad outlines, disregards two important aspects of the problem crucial to our understanding of the interplay of cultural and economic forces that created the specific forms of Indian labor in Brazil. First, not only did the value of the goods cause a crisis in the barter relationship but the nature of the goods did as well. Axes and firearms must have had a profound impact on the nature of Indian economy by transforming two of the most difficult and time-consuming tasks, tree-felling and hunting.¹⁷ Iron tools increased the effectiveness of Indian labor and reduced the amount of time needed for certain activities. Yet the change from stone to iron probably did not increase production. By enabling the Indians to satisfy their material needs more quickly, it left them with more free time to

¹⁷ Alfred Métraux, “The Revolution of the Ax,” *Diogenes*, 25 (1959): 28–40. Indian concern for firearms and ironware is apparent in a curious dialogue in Tupí and French printed as part of Jean de Lery’s account of Brazil; see *Viagem a terra do Brasil* (3d ed.; São Paulo, 1960), 251–68, in which the notes of the linguist Plínio Ayrosa are helpful.



A nineteenth-century artist imagines a scene of Portuguese and Indians trading at the beach of Jiquitaya, "where there was always the greatest commerce with the Indians of Bahia." From Anastácio de Santana (O Pardo Velho), "Guia dos Caminantes (1817)," an unpublished manuscript atlas in the Biblioteca Nacional de Rio de Janeiro. (Courtesy of Dona Lygia da Cunha of the BNRJ.)

engage in ceremonies and make war. Such a hypothesis makes the Indians appear less than "rational" in terms of economic maximization, and this is exactly the problem which underlies Marchant's explanation. Second, Marchant assumes that the Indians were "economic men" involved in a self-regulating labor market, ready to make decisions on the basis of personal or communal economic self-interest. But in many primitive "economies," production and distribution of goods are part of, and usually subordinate to, other considerations of social organization like kinship.¹⁸

Marshall Sahlins has stated this position with a precision worth quoting: "Even to speak of *the* economy in a primitive society is an exercise in unreality. Structurally, 'the economy' does not exist . . . Economy is rather a function

¹⁸ Karl Polanyi, *Primitive, Archaic, and Modern Economies*, ed. George Dalton (New York, 1968), 3-37, provides a brief introduction to the vast literature on primitive economics. Also see George Dalton, ed., *Tribal and Peasant Economies* (New York, 1967). I have quite consciously taken a "substantivist" position in the continuing controversy over the nature of primitive economies, because I feel the weight of evidence on precontact Indian cultures in Brazil does not indicate that social forms were produced out of or by the means or modes of production. When faced with a situation such as contact with Europeans, Indian societies were forced to adapt certain institutions to new economic purposes and were sometimes transformed in the process, but the divergence between Indian and European economic concepts remained great. Thus, even if the arguments of the formalists (who see all societies organized around general economic principles) or of the neo-Marxists (who see the forms of social organization in primitive societies as responses to economic needs) have some validity, the continuing disparity between Indian and European economic concepts and forms still served as a major barrier to the integration of Indians into the colonial economy. For an interesting review of the literature on this controversy, see B. Marie Perinbaum, "Homo Africanus: Antiquus or Oeconomicus? Some Interpretations of African Economic History," *Comparative Studies in Society and History*, 19 (1977): 156-78.

of the society than a structure.”¹⁹ Barter fitted, quite simply, with traditional patterns of culture, even when what was traded was the communal labor for short-term building. Plantation labor did not. Of course, Indian cultures had the capacity to adapt, but what the Portuguese demanded struck at fundamental aspects of Indian life and thought. To the Indians, agriculture was “women’s work.” Once a man had enough to eat and a few new tools and weapons, why should he want or work for more? Here was a common colonial situation, noted and commented upon in so many places. The natives—obviously capable of great exertion—were seen as congenitally lazy and undependable.²⁰ Placed on plantations, they would not work; they were given instead to sulky absenteeism or simply running away. For the Indians refused to respond to the objective conditions of the market created by the Portuguese. Thus, the modes of production established were not simply a matter of European choice but were influenced by the nature of Indian society and the internal dynamics of Indian perceptions and needs.

SLAVERY WAS NOT, IN FACT, THE ONLY FORM OF LABOR the Portuguese were willing to accept. To make Indian workers available as food producers or plantation laborers, the Portuguese attempted a variety of labor systems. The first—that employed by the colonists—was outright coercion in the form of chattel slavery. The second—that tried by the Jesuits and, later, by other religious orders—was the creation of an indigenous “peasantry” by acculturation and detribalization. The third—used by both laymen and ecclesiastics alike and often presented as the ultimate objective of the first two—consisted of the slow integration of the Indians into a capitalistic self-regulating market as individual wage-laborers. In some ways these three modes of production were stages in the history of Portuguese-Indian relations during the colonial era, but the divisions were never clearly marked and the process did not always develop at the same speed in the same direction at all times in all places. During the sixteenth and early seventeenth centuries in the northeast, the Portuguese tried all three techniques simultaneously. And the struggle between the Jesuits and the colonists resembled a clash between two differing strategies with the same goal: the Europeanization of native Americans. In economic terms it was a contest between colonists bent on the imposition of a colonial slave regime and Jesuits in pursuit of a Christianized indigenous peasantry capable of becoming an agricultural proletariat.

The conflict between colonists and Jesuits has already been the subject of intensive historical interest, and there is no need to repeat that story in detail here.²¹ Of significance, however, is that this confrontation took place within a

¹⁹ Sahlins, *Stone Age Economics*, 76.

²⁰ *Ibid.*, 86. Also see the observations of S. F. Cook in *The Conflict between the California Indian and White Civilization* (Berkeley and Los Angeles, 1976), 100.

²¹ For a review of this struggle in some detail, see my *Sovereignty and Society in Colonial Brazil* (Berkeley and Los Angeles, 1975), chap. 4: “Judges, Jesuits, and Indians,” 122–39. Also see Dauril Alden, “Black Robes versus White Settlers: The Struggle for ‘Freedom of the Indians’ in Colonial Brazil,” in Howard Peckham

specific economic and theological context which placed limitations on colonist and Jesuit alike and on the crown's response to each. The Portuguese monarchs, both Aviz and Habsburg, were morally and doctrinally impelled to recognize the "humanity" of the Indians, to take seriously the royal obligation to convert them to the Roman Catholic faith, and to prohibit, as royal subjects, their illegal enslavement. With the advocacy of the Jesuits, the crown in 1570 began to legislate against the enslavement of Indians; under the Habsburgs further restrictive legislation followed in 1595 and 1609. At each juncture, however, the crown was also faced with the economic realities in Brazil, which imposed a logic of their own. The colony's value lay in sugar production, a point which the colonists never tired of making, and sugar demanded a large labor force. The sugar planters did not yet have the capital or credit necessary to supply their need for labor entirely through the expensive transatlantic slave trade, and thus they remained dependent on indigenous workers. And, left to their own decisions, the Indians would not meet the colony's needs for a variety of cultural reasons. The crown was therefore forced to reconcile its conscience with its treasury receipts.

To solve this problem, loopholes in the legislation permitted the colonists to obtain slaves taken in a "just war." The crown, moreover, supported Jesuit-controlled mission villages (*aldeias*): if the Fathers could convert the Indians and make them available for useful activities such as growing food or working in the canefields while still preserving their freedom, so much the better. What is striking here is the strength of the colonists, especially of the sugar planters, in countering the religious and economic arguments of the Jesuits with pragmatic defenses of their own and in forcing the crown to listen. From the Indian point of view, both the Jesuit and colonist strategies were physically and culturally destructive, albeit in different ways. Colonists and Jesuits agreed on the need to introduce the Indians to "civilized" life and eventually to have them become Christian workers of benefit to the European settlements. The Portuguese, in other words, forced Indian acculturation to Western patterns at a variety of levels. But the Jesuits and the colonists differed in who each believed was better prepared to carry out the directed acculturation process. Both sides did seem to agree that change was desirable and that the ultimate goal was the creation of an indigenous population as similar to Europeans as possible.

Attempts to create an Indian peasantry basically ended in failure, although partially for reasons beyond the Jesuits' control. In Bahia the order created twelve villages (*aldeias*) in the 1550s and 1560s, with a total of about forty thousand Indians. By 1590 flight, disease, and dislocation had reduced the number of *aldeias* to three and the Indian population to only four thousand. By that date Pernambuco, the other major northeastern captaincy, had only two

and Charles Gibson, eds., *Attitudes of Colonial Powers toward the American Indian* (Salt Lake City, 1969), 19-46. For the best overall study of Portuguese Indian policy, see Georg Thomas, *Die portugiesische Indianerpolitik in Brasilien, 1500-1640* (Berlin, 1968). Also see Matias Kieiman, *The Indian Policy of Portugal in the Amazon Region, 1614-1693* (1954; reprint ed., New York, 1973).

thousand Indians under Jesuit control.²² Although the level of Jesuit activity varied from captaincy to captaincy, the policy remained the same. The Indians received an education in how to live a Christian life, a concept which included not only European morality but work habits as well. Since the Jesuits basically agreed with the colonists that Indian culture was barbaric, little attempt was made to accommodate or preserve indigenous life: unlike their actions in China and India, the Jesuits showed little cultural relativism in Brazil.²³ As far as possible, they created a full Catholic religious life in the *aldeias*.²⁴

At first glance the *aldeias* seem to provide a communal village existence parallel to the pre-European organization of life. The analogy, however, is deceptive, for the Christian communalism of the ecclesiastics bore little resemblance to indigenous patterns, especially when major integrative features of Indian culture were eliminated or transformed. Understandably, the fathers sought to exterminate such fundamental features of Tupinambá life as polygamy, marriages between uncles and nieces, ritual cannibalism, and warfare, but this policy obviously undermined traditional life. For reasons of missionary convenience, moreover, the villages very quickly lost their cultural integrity as peoples of various tribal groups and tongues were mixed together. The Jesuits used *lingua geral*, a simplified form of Tupí, as a *lingua franca* and thereby further reduced Indian culture to a common base that could be controlled by the fathers. Such methods made preaching and conversion easier but increased the pace of detribalization. By their actions the Indians demonstrated how they felt about this assault on their culture. Despite the rosy glow of missionary zeal that pervades much of their comments on the *aldeias*, many Jesuits recognized flight as a chronic problem. In 1566 Father Inácio de Azevedo wrote that "many Indians wish to go with them [colonists] and serve them rather than live in the *aldeias*."²⁵ The Indians clearly did not always perceive the protection of the Jesuit *aldeias* as necessarily preferable to the rigors of plantation slavery.

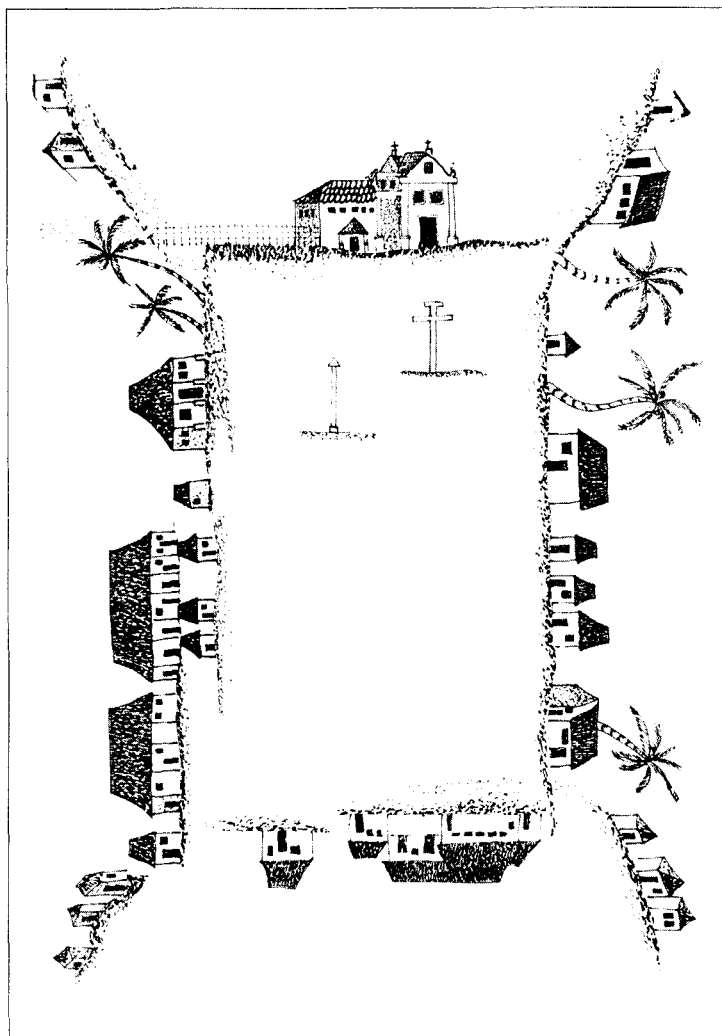
The effect of intentional European interference in traditional practices was compounded by more subtle disruptions in the *aldeias*. Their physical plan, for example, was European. The Jesuit *aldeias* were physically organized according to European norms with a central plaza, a church, and rows of house units flanking the open space. Tupinambá village organization with its layout of four to eight long houses shared by many related families was quite different;

²² HCJB, 2: 58–59. Also see Francisco Soares, *Coisas notáveis do Brasil* (Rio de Janeiro, 1966), 71. Soares' account dates from 1589. Also see Eduardo Hoornaert et al., *História da igreja no Brasil primeira época* (Petropolis, 1977), 128–31.

²³ Thales de Azevedo, "Catequese e aculturação," in his *Ensaio de antropologia social* (Bahia [1957?]), 33–62. For example, the following statement is typical: "Como a gente era rude y sin ninguna policia humana," in "Historia dos collegios do Brasil," *ABNR*, 19 (1897): 75–144. Also see Vasconcellos, *Chronica da Companhia de Jesu do Estado do Brasil*, 1: lxxv.

²⁴ Cardim, *Tratados de Terra e gente* (1583) (Rio de Janeiro, 1925), 280. Also see Letter of Father Antônio Pires (Pernambuco: June 5, 1552), *Cartas Jesuiticas*, vol. 2: Afranio Peixoto, ed., *Cartas Avulsas, 1550–1568* (Rio de Janeiro, 1931), 124.

²⁵ (Bahia; November 19, 1566) *MB*, 4: 369–70. Also see HCJB, 2: 73; and Hoornaert, *História da igreja no Brasil primeira época*, 131.



The Jesuit Aldeia de Espírito Santo, which became the town of Abrantes, Bahia, in 1758. (The drawing is based on an original in the Arquivo Histórico Ultramarino, Lisbon.)

the complex organization of the Gê-speakers with the village divided into moities and clans, with separate residences for certain age and sex groups was even farther still from the European plan. The traditional village and residence patterns reflected the Indian social and religious cosmos. To change those patterns was to breach the security of the traditional universe and to disorient the Indians in the literal meaning of the word. As Claude Levi-Strauss has pointed out in another context, "All feeling for their traditions would desert them, as if their social and religious systems . . . were so complex that they could not exist without the schema made visible in their ground plans and reaffirmed to them in the daily rhythm of their lives."²⁶

²⁶ Plans of some Jesuit *aldeias* are preserved in the AHU, seção de iconografia. For a reproduction of the plan of the Vila de Abrantes in Bahia, see Nestor Goulart Reis Filho, *Evolução urbana do Brasil (1550-1720)* (São Paulo, 1968), fig. 23. And Levi-Strauss, *Tristes Tropiques* (New York, 1961), 203-05; this discussion focuses on the intentional modification of a Bororo village by Salesian Fathers.

From the Jesuit viewpoint, of course, the success of the *aldeias* was measured by the degree to which Indian culture was modified. The Jesuits argued that the *aldeias* not only protected the Indians from slavery and facilitated their conversion but provided an auxiliary military force for use against hostile tribes, foreign interlopers, and restive slaves. "And work for the Jesuits for nothing," muttered the colonists. The fathers of the Company claimed in response that the *aldeias* also provided labor and sustenance to the estates of the colonists. By 1600 the Jesuits claimed to have fifty thousand Indians in the *aldeias* of Brazil available to both crown and colonists.²⁷ What disappeared relatively early from the Jesuits' defense of their Indian policy is the creation of an indigenous peasantry.

As Portuguese colonization became increasingly based on export agriculture, conflict with the Indians over land became a central problem. Jesuits directed their efforts toward the protection of village lands and the restoration of property seized illegally. Land grants (*sesmarias*) were given to the Jesuit *aldeias* to insure the Indians against starvation and to provide a basis for Indian production of foodstuffs.²⁸ Generally, Indian *aldeias* and lands were not located in the regions of the rich *massapé* soils considered ideal for sugar production. Colonists in Bahia claimed in the early seventeenth century that Jesuit *aldeias* could produce over thirty thousand bushels of manioc flour a year. By that time, however, the struggle between Indian subsistence agriculture and the export crops was clearly defined. In 1610, the town council of Paraíba argued that, since sugar was the basis of the Brazilian economy, "the harm caused by their [Indian] farming is much greater than any benefits that might accrue."²⁹

To create an indigenous peasantry in Brazil, the Portuguese were forced to start from scratch. In Brazil, unlike Yucatan or the Andean highlands, there was no pre-Colombian tradition of communal agriculture linked to a larger state system. As in Paraguay, the Jesuit *aldeias* of the Brazilian northeast attempted to create peasant communities where none had existed—at least not in ways that would serve the interests of the colony. The Jesuits were not only the defenders of the Indian communities, they were also the creators of them.³⁰

With the support of the crown and of sympathetic administrators like Governor General Mem de Sá, the Jesuits were able to secure lands for their Indian charges, but the Jesuit attempt to create a peasantry that was not only

²⁷ AGS, Secretarias provinciales, 1461, ff. 104–08.

²⁸ Letter of Nóbrega (May 8, 1558) in Serafim Leite, ed., *Cartas do Brasil e mais escritos do P. Manuel da Nóbrega* (Coimbra, 1955), 292. In 1558 Father Nóbrega sought a grant of land to the *aldeia* São Paulo (in present-day Brotas, a suburb of Salvador). He felt that the owner of the land, the count of Castanheira, would cede the property since, lacking water for an *engenho*, it served for very little. Not even Nóbrega would have tried to secure lands for the Indians which could be used for sugar cane cultivation.

²⁹ *HCJB*, 5: 21. ANTT, *Corpo Cronológico*, p. 1, maço 115, no. 108.

³⁰ Regimento of Tomé de Sousa, *DHA*, 1: 53. Also see the discussion in Marchant, *From Barter to Slavery*, 90–91. For an interesting theoretical discussion, see Juan Carlos Garavaglia, "Un modo de producción subsidiario: La organización económica de las comunidades guaranizadas durante los siglos xvii–xviii en la formación regional altoperuana-rioplatense," in *Modos de producción en América Latina* (Cordoba, Argentina, 1973), 161–92.

self-supporting but also a supplier of the colony's needs was never realized. The first royal governor, Tomé de Sousa, arrived in 1549 with specific instructions for establishing a weekly market where Portuguese and Indians could conduct business. This system was designed to supply the food requirements of the Portuguese and, at the same time, protect the Indians from the worst aspects of extortion and fraud by prohibiting the colonists from entering villages at will. But even at the outset concessions were made to the planter class, for only the planters and their people were allowed to trade with the Indians for food whenever it was convenient. Voluntary supply failed, in part because the Indians would not respond to the conditions of the labor market. Aside from the royal instructions (*regimento*) of Tomé de Sousa, there are almost no other references to the weekly fair he was supposed to have established. Lands were given to the Indians primarily to assure their livelihood and therefore their availability to the colony. Mem de Sá stated this position clearly in justifying his grant of a *sesmaria* to the Aldeia de Espírito Santo in Bahia: "seeing how beneficial and necessary they are to this Bahia and that they cannot sustain themselves without lands to farm. . . ." ³¹

Neither the sugar planters nor the monarchs were anxious to recognize the failure of an indigenous peasantry. In the ideal mental landscape of sugar producers, the sugar plantations (*engenhos*) would be ringed by canefields as far as possible and at the outer limits by villages of "domesticated Indians," who would keep out the unreduced tribes beyond while growing large quantities of manioc and other foods. Of course, if the Indians worked at the *engenho* on occasion, that was all the better. Even at the end of the sixteenth century the crown still made reference to the benefits of having Indians farming close by the *engenhos* of the Portuguese. Certainly, the *engenho* population sometimes acquired foodstuffs from Indian growers, but this was on an intermittent and haphazard basis. Eventually, what emerged was a geographical division between the export crop, sugar, and the locally consumed food crops. Large-scale production of manioc was pushed into areas that could not profitably support sugar agriculture. The peasantry that developed to work this crop was not Indian, but a predominantly mixed population of *mestiços* and mulattoes. ³²

After failing to create an indigenous peasantry, the Jesuits justified their continued control of the villages by emphasizing the military and labor services which their charges provided. In Bahia during the early 1580s the Jesuit *aldeias* supplied some four to five hundred workers to the colonists under a system of contract labor. The Indians received a meager wage of four hundred réis per month (scarcely a third of a common boatman's salary), but even this sum was often never paid. The Jesuits tried to limit the time of

³¹ *MB*, 3: 530–31. On Mem de Sá's support of the Jesuits, see Herbert Ewaldo Wetzel, *Mem de Sá: Terceiro Governador geral* (Rio de Janeiro, 1973), 205, 215–17.

³² *Alvará* (August 21, 1587), in *DIA*, 1: 321–22. Shepard Forman and Joyce Riegelhaupt, "Bodo Was Never Brazilian: Economic Integration and Rural Development among a Contemporary Peasantry," *Comparative Studies in Society and History*, 12 (1970): 188–212; and Francis Jennings, *The Invasion of American Indians, Colonialism, and the Cant of Conquest* (New York, 1975), 58–85.



Indians under European control in Colonial Brazil; an *aldeia* is mobilized to fight for the Dutch. From Joan Bleau, *Le grand atlas ou cosmographie Blaviane* . . . , volume 12: *Amerique* (Amsterdam, 1667), between 263 and 264. (Courtesy of the James Ford Bell Library, University of Minnesota.)

service to three months and usually refused to permit women to accompany the men. Such restrictions made the planters uncomfortable with Jesuit control over their laborers.³³

Sugar planters were certainly not averse to hiring wage laborers if they could be obtained in sufficient numbers and under conditions favorable to the employers. As early as 1561 settlers in Bahia had sought to hire Indians for wages (*soldada*), and the accounts of Engenho Sergipe indicate that Indians did provide services for pay, although usually at a rate far below that of whites, free blacks, and mulattoes.³⁴ The royal Indian legislation of 1596 was a clear demonstration of the crown's attempt to integrate the "tame" Indians (*indios mansos*) into the colony as wage laborers. The law also stated unambiguously that Indian laborers were not to remain on the engenhos for more than two months of continual service and that no advance payments were to be made to them. This restriction is a hint that planters may have been turning to a form of debt peonage as an answer to their labor needs. The crown forbade either Jesuits or colonists from using Indians unless they were paid "like free men and treated as such."³⁵ We will never know whether *aldeia* resi-

³³ Soares, *Coisas notáveis do Brasil*, 77. The wages paid to a boatman in 1574 at Engenho Sergipe were 16\$500 réis or an average of 1\$375 per month. See Mircea Buescu, *300 anos da inflação* (Rio de Janeiro, 1973), 60–61. *HCJB*, 2: 94–95.

³⁴ *Moradores* of Bahia asked in 1561 that the probate judge "desse a soldada os moços e moças orfãos e outros pedião os casados." Padre Luís de Grã to Padre Miguel de Torres (Bahia; September 22, 1561), in *MB*, 3: 431. On Indian workers at Engenho Sergipe, see Paul Silberstein, "Wage Earners in a Slave Economy: The Laborers of a Sugar Mill in Colonial Brazil," unpublished paper, University of California, Berkeley (1970).

³⁵ *DHA*, 1: 404–05.

dents or other Indians within the radius of Portuguese control could have filled the labor demands of the sugar industry as free laborers. The planters were unwilling to pay wages sufficient to create an adequate wage-labor market so long as an alternate type of cheap labor was available in the form of Indian slaves.³⁶ That the Jesuits would not permit Indians to neglect their own farming in order to work for the colonists—for fear that starvation would result—indicates that the salaries paid were insufficient to meet necessary minimum requirements of subsistence.³⁷

The period of intense Jesuit activity in coastal Brazil roughly coincided with the apogee of Indian slavery. By 1545 the southern captaincy of São Vicente had six *engenhos* and three thousand slaves, almost all of them Indians. Indian captives could also be found on the plantations of Pernambuco, Bahia, and Porto Seguro. In 1583 Pernambuco had sixty-six *engenhos* and some two thousand African slaves. Since an *engenho* probably drew on the labor of one hundred slaves, Indians still accounted for two-thirds of Pernambuco's *engenho* work force even during a period of transition to African labor.³⁸

In Bahia the establishment of royal government in 1549 considerably aided the expansion of the sugar economy. Enslavement of the local tribal groups accompanied this expansion. In the 1550s a number of military campaigns were carried out in the Recôncavo. Both Duarte da Costa and his successor, Mem de Sá, protected established *engenhos*, secured land for new mills, and obtained captives in a series of punitive expeditions carried out by the Portuguese and their Indian allies.³⁹ In Pernambuco and Bahia as in other captaincies, the colonists (*moradores*) obtained Indian slaves by "ransoming" (*resgate*) them from other Indians who had already taken them as war captives. More common, however, were Portuguese raids (*saltos*) made for the specific purpose of obtaining slaves. Denounced by the Jesuits and the crown as unlawful and prohibited in the regimento of Tomé de Sousa and his successors, *saltos* remained the most prevalent form of enslavement. As one Jesuit put it, "only by a miracle does one find here a slave not taken by assault."⁴⁰

The practice of encouraging free Indians (*forros*) to marry or accompany those already enslaved reflects the plantations' increasing demand for Indian workers of any type. This tactic enlarged the supply of labor but was severely denounced by crown and clergy alike. In 1566 the Jesuits were able to obtain legislation against the practice,⁴¹ but *engenho* inventories of the 1570s and

³⁶ Marvin Harris, *Patterns of Race in the Americas* (New York, 1964), 20–21. Harris follows the argument of John Phelan that Indians "took to earning a living European fashion when adequately compensated." It should be remembered, however, that Phelan is referring to highland peoples already integrated into larger state structures before the arrival of the Europeans. See his "Free versus Compulsory Labor: Mexico and the Philippines, 1540–1648," *Comparative Studies in Society and History*, 1 (1958–59): 189–201.

³⁷ *HCJB*, 2: 95.

³⁸ Marchant, *From Barter to Slavery*, 131, based on a number of contemporary sources.

³⁹ J. F. de Almeida Prado, *A Bahia e as capitanias do centro do Brasil*, 2 vols. (São Paulo, 1945–48), 2: 7–123, contains a survey of these actions. The expedition commanded by Mem de Sá—the War of Paraguassú—was caused to some extent by the fact that Indians in that region refused to return runaway slaves to the Portuguese. Also see Leite, *Cartas Nóbrega* (Bahia; July 5, 1559), 343; and Wetzel, *Mem de Sá*, 59–68.

⁴⁰ *HCJB*, 2: 96, 194.

⁴¹ "Resoluções da Junta da Bahia sobre as aldeias dos padres e os Índios," *MB*, 4: 354–57.

seventeenth-century wills from São Paulo indicate that the practice of considering forros as little different than slaves did not disappear easily.⁴²

Jesuit criticism of the enslavement of Indians on the sugar plantations is well known. The Jesuits denounced immorality, concubinage, lack of religious instruction, and brutal treatment, but they did not deny the necessity of Indian labor.⁴³ The question was how best to use them, under whose control, and under what conditions. The colonists generally argued that Indians in contact with Europeans would naturally become Christians and would learn skills of use to the colony, that there was little need for specific Jesuit supervision.⁴⁴ Neither Jesuits nor colonists were prepared for the persistent Indian resistance to a European work regime and for the disastrous demographic decline of the Indian population of the 1560s.

Brought into close contact with Europeans in *aldeias* and *engenhos*, the Indians suffered the ravages of European diseases. In 1559 or 1560 smallpox killed over six hundred enslaved Indians in Espírito Santo alone; and no one knew the number of dead among free Indians. By 1561 it had spread to the Recôncavo of Bahia. The epidemic reached its height in 1563. Estimates placed the figure at thirty thousand dead among the Indians under Portuguese control in Bahia alone, to say nothing of countless more in the *sertão* to which the disease was spread by Indians fleeing from the coast.⁴⁵ Father Leonardo do Valle, an eyewitness, wrote of children who died at their mother's breast for lack of milk, of people so debilitated that they could not even dig graves for the dead or draw water for the living. One-third of all the Indians in the Jesuit *aldeias* died. Those on the sugar estates suffered similar losses. On some plantations ninety to one hundred slaves perished.⁴⁶ The following year brought no respite. In 1563 a second epidemic, this time measles (*sarampo*), struck an already weakened population. Perhaps another thirty thousand died.⁴⁷ Not surprisingly, measles proved far more lethal to Indians than to Portuguese. Europeans were stunned by its effect: "the population that was in these parts twenty years ago is wasted in this Bahia, and it seems an incredible thing for one never believed that so many people would ever be used up, let alone in such a short time."⁴⁸

The epidemics of 1560–63, not surprisingly, disrupted the social and economic fabric of the colony. Portuguese concentration on the export crop of

⁴² *DHA*, 2: 64, 350. Edmundo Zenha, *Mamelucos* (São Paulo, 1970), 130–34; and Alcântara Machado, *Vida e morte de um bandeirante* (São Paulo, 1943).

⁴³ See, for example, the pertinent sections of *HCBJ*, especially vols. 2–3, 5.

⁴⁴ Diogo de Campos Moreno, *Livro que da razão as estado do Brasil* (Rio de Janeiro, 1968). See Engel Sluiter, ed., "Report on the State of Brazil, 1612," *Hispanic American Historical Review*, 29 (1949): 523. The classic statement of the colonist position is presented in Serafim Leite, ed., "Os capitulos de Gabriel Soares de Sousa," *Ethnos*, 2 (1941): 5–36.

⁴⁵ For a brief but accurate account of the plague, see Fernandes, *Organização social Tupinambá*, 40–41. Also see *Cartas Avulsas*, 207–08.

⁴⁶ Leonardo do Valle to Brothers of the Company (Bahia; September 23, 1561), *Cartas Avulsas*, 334. Vasconcellos estimated that three-quarters of all the Jesuit Indians died; see his *Chronica da Companhia de Jesu do Estado do Brasil*, 3: 6. Also see *Cartas Avulsas* (Bahia; May 12, 1563), 378–93.

⁴⁷ Fernandes, *Organização social Tupinambá*, 40. "História dos collegios do Brasil," 84, places the plague in 1563–64.

⁴⁸ Father José de Anchieta, as quoted in Fernandes, *Organização social Tupinambá*, 30.

sugar and their dependence on indigenous supplies of foodstuffs had, even in the best of times, created an unstable situation. Now with the decimation of the Indians, the main sources of food supply were radically depleted and famine set in. The Portuguese went hungry and the Indians starved to death. Some Indians faced with starvation sold themselves into bondage rather than perish. If those who took this course believed that servitude would be temporary, they soon discovered that such was not the case.⁴⁹ Although these vital crises of the 1560s in some ways facilitated the enslavement of Indians still within the range of Portuguese operations, epidemics and famines also made clear the dangers inherent in dependence on Indian labor.⁵⁰

Continuing Indian resistance to Portuguese settlement and to the plantation labor system provided the colonists with further proof of the problems of employing an indigenous labor force under a colonial regime. Indian resistance took a number of forms. Primary resistance of unreduced groups who sought to protect traditional territories characterized the period from 1540 to 1580 as the lands brought under sugar cultivation expanded. Indian opposition was eliminated in some areas by Portuguese military campaigns as the Indians were driven into the interior, enslaved, or killed. Resistance did not cease, however, once the Indians were brought under Portuguese domination. In 1567 a general slave insurrection swept the region of the Bahian Recôncavo. Masters were killed and slaves fled the canefields in large numbers. Only the intervention of Jesuit-led *aldeia* Indians brought the situation under control.⁵¹ More lasting was a large-scale millenarian resistance movement called *Santidade*, which flourished among escaped Indian slaves and former *aldeia* inhabitants in the region of southern Bahia. Combining Roman Catholic and native beliefs, the *Santidade* followers began burning sugar mills and plantations in the 1560s, and despite Portuguese military reprisals their activities continued into the seventeenth century. In 1610 their numbers were reported at twenty thousand and included escaped blacks as well. As late as 1627 their raids continued in the southern Recôncavo and served as a beacon to those still in captivity.⁵²

⁴⁹ Pero de Magalhães de Gandavo, *The Histories of Brazil*, trans. John B. Stetson (New York, 1922), 229. A similar situation occurred in Pernambuco in 1583–84 when a famine in the *sertão* drove three to four thousand Indians onto the coastal plantations.

⁵⁰ The susceptibility of Indians to European disease continued. In 1565 so many died in the Jesuit Aldeia de São João in Espírito Santo that the site had to be abandoned; *MB*, 4: 267–68. In 1616–17 Indian and African slaves alike were decimated by smallpox; see Brandão, *Diálogos das Grandezas do Brasil*, 64. The impact of disease in the *aldeias* was not lost on the colonists: in 1610 the Câmara of Paraíba argued against the *aldeias* for exactly this reason; ANTT, *Corpo Cronológico*, p. 1, maço 115, no. 108.

⁵¹ “História dos collegios do Brasil,” 89.

⁵² For the most complete study to date, see José Calasans, *A santidade de Jaguaripe* (Salvador, 1952). This account and all others have been based on the sixteenth-century materials contained in the Jesuit letters and the Inquisition records. The short account here, drawn from my forthcoming book, presents for the first time evidence of the existence of the movement in the seventeenth century. See Diogo de Meneses to the crown (Bahia; September 1, 1610), ANTT, Fragmentos caixa 1, n. 6; the king to Gaspar de Sousa (Lisbon; January 19, 1613 and May 24, 1613), BI, Correspondência de Gaspar de Sousa, ff. 183–85v., 218–18v.; and Provisão de Diogo Luis de Oliveira, Arquivo da Câmara Municipal do Salvador, Provisões e portarias 1624–42, bk. 155, ff. 243–46.

The colonists were not ready to abandon Indian labor for the growing number of sugar mills in the late sixteenth century, but the uncertainties of Indian resistance and their health and life expectancy made indigenous labor an investment of great risk. This situation helps to explain why Indian slaves were priced far below Africans, why planters were more likely to invest in the training of Africans for the skilled tasks in the sugar mills, and why the colonists did not entirely oppose the development of a wage labor system. The 1570s and 1580s witnessed not only military expeditions but also a number of other schemes designed to bring still unreduced Indians from the interior to meet the labor needs of the *engenhos*.⁵³ But, faced with the increasing opposition of the crown to enslavement, the growing demands of the sugar economy, and the disastrous example of the 1560s, the colonists turned to the labor to be found in the Atlantic slave trade. It is no accident that the importation of large numbers of Africans began in the 1570s following the peculiar conjunction of demographic, economic, and political circumstances that demonstrated the risks of an economy based on captive or coerced Indian labor.

The use of Indian labor was, therefore, subject to a number of constraints and limitations. The deadly triad of warfare, disease, and famine that accompanied the conquest and occupation of Brazil limited the nature and availability of an indigenous labor force. The competing strategies of the Jesuits and the colonists over the form and control of this labor system determined much of the history of Portuguese-Indian relations, but this rivalry should not mask the basic agreement shared by planters and missionaries alike that Indian labor was vital to the colony's success. Each side justified its position to the crown by arguing that its control would more rapidly lead the Indian to European standards of religion, morality, and habit, including integration of the Indian into a wage-labor market. But most Indians refused to respond to either: they refused to be shaped at will by alien policies and historical processes no matter how seemingly inexorable. Indian actions and responses varying from armed resistance to accommodation and acculturation limited and defined the nature of the colonial regime. By examining the inner structure of these processes in relation to the establishment of the plantation regime, in relation to the formation and definition of the colony's dominant mode of production, it is possible to define that regime and to suggest the reasons for the abandonment of Indian slavery in the New World's first successful plantation colony and economy.

⁵³ Between 1571 and 1575, about twenty-five thousand Indians were brought under Portuguese control in Bahia. *HCJB*, 2: 82–83. The duke of Aveiro, donatary of Porto Seguro, received permission to “bring down” Indians as did the Count of Linhares but these were exceptions. See ANTT, *Cart. Jesuítas*, maço 16; AGS, Sec. prov. 1487 (October 7, 1603), ff. 33–33v. In 1608–10 Governor Diogo de Meneses favored colonists' desires to bring Indians under plantation control, but the crown prohibited such efforts in 1613. Also see *ABNR*, 37 (1939): 37–40; BI, Correspondência Gaspar de Sousa (March 25, 1613), f. 207. On the famous case of the Potiguares of Pernambuco sent as allies and workers to Ilhéus, see Robert Southey, *History of Brazil*, 2 vols. (London, 1810), 1: 404–05; ANTT, *Cart. Jesuítas*, maço 8, n. 108; and Frei Vicente do Salvador, *História do Brasil* (São Paulo, 1968), bk. 4, chap. 35.

MOST OF THE DOCUMENTATION and thus the historiography of Indian labor and slavery has, unfortunately, been concerned with the legality and the abuses of the system. Extant but little-used plantation records and parish registers offer another option.⁵⁴ The forms, usages, and structures of Indian labor as practiced on the plantations of northeastern Brazil can provide a key to our understanding of the process by which a colonial—Indian supplanted by African—slave regime became essential to a tropical plantation economy.

The terminology of Indian labor is, in itself, revealing of its position within the plans and perceptions of the Portuguese. The categories of social definition and of social structure in Brazil were determined to a great extent by the nature of the agricultural enterprise and by the previous Continental and overseas experience of the Portuguese. Whatever the philosophical and theological problems provoked in Europe by the discovery of a new “race” of men, the Portuguese on the scene in Brazil tended to draw upon familiar models, especially the recent past of African contacts and Atlantic plantations. This tendency is revealed in the widespread use of “*negro da terra*” by Jesuits as well as colonists to describe Indians.⁵⁵ “*Negros da terra*” was a parallel phrase to the description of Africans as “*negros de Guiné*.” In medieval Portugal the word “negro” itself had become almost a synonym for slave, and certainly in the sixteenth century “negro” carried implications of “servitude.” The phrase therefore reveals Portuguese perceptions of both Africans and Indians, not so much of skin color as of their relative social and cultural position vis-à-vis the Portuguese. Over the course of the sixteenth century, “*negro da terra*” slowly disappeared from common usage as more and more Africans were introduced into the colony. It vanished, in fact, with Indian slavery itself.

For those Indians not enslaved but still under Portuguese control and direction, the colonists applied a variety of terms. Such people were *índios aldeados* (village Indians) or *índios sob a administração* (Indians under the control of . . .) or, most commonly, *forros*. This last term is somewhat confusing

⁵⁴ One of the reasons why this topic has not been studied in greater depth is the lack of materials. Historians of sixteenth-century Brazil depended to a large extent on Jesuit letters and reports and on government correspondence and legislation. For much of the discussion thus far, I have done the same. Jesuit observers and colonial officials usually wrote of general conditions, not the specifics of slavery in sugar agriculture. For this reason, the existing materials from Engenhos Sergipe and Santana are particularly valuable. Extant inventories from 1572–74, 1591, and 1638 that include lists of slaves enable us to come to some conclusions about the structure of the labor force. Account books of individual harvests from 1607 to 1748 also exist in the Arquivo Nacional da Torre do Tombo, Lisbon. And Engenho Sergipe’s chapel served, until the late seventeenth century, as the parish church of its district, and the chapel register has survived, albeit in fragmentary condition; it permits us to examine various social relationships in the period from 1595 to 1626. These sources, limited as they admittedly are, at least provide a glimpse of life on Bahian engenhos in the period of Indian slavery. Whenever possible, supporting materials have been used to confirm the findings of the Sergipe and Santana documents.

⁵⁵ The term “*negro da terra*” seems to have persisted in São Paulo well into the seventeenth century. In Bahia, although it was occasionally employed in the period after 1600, it was gradually replaced. See Zenha, *Mamelucos*, 52–72. For an introduction to the question of Indian labor in the sugar industry, see Luís Viana Filho, “O trabalho do engenho e a reação do índio Estabelecimento de escravatura africana,” *Congresso do Mundo Portugês*, 10 (Lisbon, 1940): 10–29. Also see Gilberto Freyre, *The Masters and the Slaves* (New York, 1956), 81–184.

since it was also used to describe a slave who had gained his freedom through manumission (*alforria*), but in sixteenth-century Brazil it was not used exclusively in this way. Instead, *indios forros* were not only freedmen but also Indians who were not enslaved and were under Portuguese control—especially, though not exclusively, that of the Jesuits. The engenhos made use of all three categories of Indians during the sixteenth century and continually sought to increase the pool of available labor. At various times colonists sought the creation of the *encomienda*, the system of personal award of Indian service or tribute as was done in Spanish America. Although never formally instituted, the various arrangements for the use of “free” Indian workers amounted to a system of labor supply parallel to the *repartimiento* of the Spanish colonies.⁵⁶

Aside from the supply of labor from the *aldeias*, the plantations acquired Indians by enslavement, barter, and wages. The law of 1570 had prohibited the legal enslavement of indigenous peoples, but loopholes in the legislation permitted the acquisition of captives by ransoming them through trade with their captors. This trade (*resgate*) was theoretically designed to save those already destined for a cruel death at the hands of their traditional enemies. Rescue was a favor, to be repaid by the captive’s labor. Abuses were both obvious and widespread, but, “because of the necessity that the estates have for Indians,” few in the 1570s wanted to restrict *resgate*. Individual plantations often arranged for the acquisition of workers by this means. In 1574, for example, Engenho Sergipe had over fifty Indians recently brought in by a *resgate* expedition, and an inventory of the property from about that time contains references to axes, cloth, and knives specifically intended for such trade.⁵⁷ *Resgate* enabled the Portuguese settlers to obtain *negros da terra* without having to call them slaves, and it permitted the continuance of slavery long after its apparent abolition.

By the 1580s royal legislation and the increasing effectiveness of the Jesuits had begun to create problems for those who wished to obtain Indians by both *resgate* and “just war.” After a visit to the captaincy of Bahia during 1588 and 1589, Jesuit *Visitador* Cristóvão de Gouvea recommended that the Church refuse the sacrament of confession to anyone involved in the *resgate* of Indians. Jesuit effectiveness in overseeing the activities of free Indians within the Portuguese sphere of influence, moreover, began to create difficulties for the colonists. In 1589, Ruy Teixeira, administrator of Engenho Sergipe, complained to his absentee employer, the Count of Linhares, that new legislation had made the Jesuits “masters of the land and of the Indians who with the title of ‘forros’ serve them, being in reality more captive than the Guiné slaves.” He lamented that no Indians remained for *resgate*, but he realized that nothing could be

⁵⁶ For some data on this problem, see my *Sovereignty and Society in Colonial Brazil*, chap. 4. Also see Thomas, *Die portugiesische Indianerpolitik in Brasilien, 1500–1640*, 90–98.

⁵⁷ As quoted in Wetzel, *Mem de Sá*, 216–17. The inventories of Engenhos Sergipe and Santana made between 1572 and 1574 are printed along with the will and testament of Mem de Sá and other relevant materials in *Documentos para a História de Açúcar*, vol. 3: see Inventário Engenho Sergipe (1572), *DHA*, 3: 65; and Livro de Contas do procurador (1574), *DHA*, 3: 406.

done. "I will speak no more of this for these are matters that have no remedy—may God grant us His mercy."⁵⁸

A variety of labor arrangements for Indians existed simultaneously on the Brazilian plantations, and, although their relative importance varied with time and place, forms of remunerated labor increasingly replaced slavery. The owner of Engenho Sergipe paid the tithe for a nearby *aldeia* that provided workers while using slaves and other free Indians.⁵⁹ In addition to the local labor pool, some Indians from the interior were forced by hunger or military pressure to present themselves for work at the plantations.⁶⁰ Indians continued to participate as laborers in the plantation economy and life of the colony, (although their presence is largely ignored in traditional historiography, which emphasizes the role of African labor).⁶¹ The planters tended to employ these workers according to their status or manner of acquisition. Slaves were employed in those tasks central to the sugar-making process where returns to investment were highest, regardless of skills required; the contention that slavery cannot be profitable in production that demands skilled labor is, therefore, not supported by the data on Indian plantation slavery in Brazil.⁶² Evidence from Engenho Sergipe demonstrates that "free" or *aldeia* Indians were used primarily as an auxiliary work force in tasks of maintenance or service peripheral to the business of sugar production. They cleaned and repaired the water system, worked in the boats, hunted and fished, and cut firewood.

Access to nonslave Indian labor allowed the planters to concentrate the capital invested in slaves in those crucial aspects of production where continuous labor justified the fixed capital the slaves represented. The almost ten-month harvest season (late July through May) and the relatively mild rainy period meant that slaves could work during the entire year in northeastern Brazil.⁶³ Thus, the conditions of, rather than any imagined lack of complexity

⁵⁸ "O que pareceo ao Padre Visitador Cristóvão de Gouvea ordernar na visita deste Collegio da Bahia," January 1, 1589, Biblioteca Nazionale di Roma, Fondo Gesuitico 1367; and Teixeira to the count of Linhares (Bahia: March 1, 1589) ANTT, *Cart. Jesuitas*, maço 8, no. 136.

⁵⁹ Sebastião Vaz to Diogo Cardim, Provincial of the College of Santo Antônio, Bahia, June 3, 1629, ANTT, *Cart. Jesuitas*, maço 69, no. 74. This letter describes the history of Indians in the village near Sergipe do Conde. They had been brought in at great expense by the count of Linhares but, by the time his wife and heir had died, there were few left. When the village was moved elsewhere, however, the engenho Indians went along, a situation that moved Vaz to petition for their return. Also see Safra 1611-12, ANTT, *Cart. Jesuitas*, maço 14, no. 4, f. 24; and *DHA*, 3: 102, 298, 311, 406.

⁶⁰ *Fetor* of Engenho Santana to the count of Linhares, August 15, 1599, ANTT, *Cart. Jesuitas*, maço 8, no. 105. The *fetor* called them "gentio do sertão tapuyas do catingua."

⁶¹ For example, see the account of Peter Carder (1578) in Samuel Purchas, *Hakluytus Posthumous: or, His Pilgrims*, 5 vols. (London, 1625), 3: 1190. Although Indians did not come in to make depositions, except in one instance, many who appeared before the Inquisitors spoke of their relations with Indians, of the *aldeias*, of entradas to the *sertão* to bring more Indians down to the coast, and of considerable interaction. A number of mestiços also admitted to practicing Indian customs and of speaking Indian languages. J. Capistrano de Abreu, ed., *Primeira visitaçõ do Santo Ofício as partes do Brasil: Confissões da Bahia, 1591-92* (Rio de Janeiro, 1935); see, for example, 34-37, 64-65, 93-97, 104-05, 123-24, 164-72.

⁶² Wallerstein, *The Modern World System*, 88-90. Manuel Moreno Fraginals, *The Sugar Mill*, trans. Adric Beltrage (1964; English ed., New York, 1976), presents a developed analysis based on this classic Marxist premise. But Robert S. Starobin, *Industrial Slavery in the Old South* (New York, 1970), for example, demonstrates that there was no necessary incompatibility between slavery and highly technical operations.

⁶³ Compare Wallerstein, *The Modern World System*, 88-90, and Ralph Anderson and Robert E. Gallman, "Slaves as Fixed Capital: Slave Labor and Southern Economic Development," *JAH*, 64 (1977): 24-46.

TABLE 1
ETYMOLOGY OF SELECTED TUPI PERSONAL NAMES
ENGENHO SERGIPE, 1572-74

<i>Name</i>	<i>Probable Derivation</i>
Pejuira	<i>peju</i> = to blow; <i>ira</i> = to detach (interrog.)
Pedro <i>rari</i>	<i>rari</i> = to be born
Itaoca	<i>Ita</i> = stone; <i>oka</i> = house
Ocaparana	<i>oka</i> = house; <i>parana</i> = sea
Mandionaem	<i>Nhae</i> = pan; <i>mandio</i> = manioc
Antonio Jaguare	<i>jaguare</i> = iaguara = jaguar
Francisco tapira	<i>tapira</i> = tapiira = ox
Birapipo	<i>Bira</i> = ybyra = wood; <i>pipo</i> is an interrogative
Cunhamocumarava	<i>kunhamuku</i> = a girl of marriageable age; <i>marava</i> = marabi = child of an Indian and a stranger
Ubatiba	<i>Tyba</i> is a plural ending; <i>Uba</i> = port, thighs, fish roe

SOURCE: *DHA*, 3: 89-103.

in, sugar agriculture are more useful in explaining the planters' preference for slaves. But planter calculations and preferences were only part of the equation. There remained the problem of Indian adaptation and adjustment to the engenho regime. The records of Engenhos Sergipe and Santana provide us with a glimpse, albeit fragmentary, of this process and the constraints it placed on actions of the sugar planters.

Indian slave names in the engenho inventories yield some tentative conclusions about the ethnic composition of the Indian slave force of Bahia in the late sixteenth century. As expected, many listed in the Sergipe and Santana inventories of 1572-74 were Tupinambá, native to the coastal area of Bahia. Some carried further references by location—such as *tapariqua* (Itaparica Island), Tamamaripe, *tapecuru* (Itapicuru), and *peroacu* (Paraguassú River)—or by common Tupi-guaraní descriptive terms—such as “*açu*” (big) or “*merim*” (small). Other names clearly seem to be of Tupinambá origin. Table 1 presents some of the more obvious Tupi names.

A variety of problems, some of which themselves reveal facets of engenho life, beset etymological analysis. Aside from the usual difficulty of parallel words in more than one language, the Portuguese who recorded these names transformed them into the sounds and orthography suitable to a Romance language. What remains is what a Portuguese heard, not what an Indian said. Sometimes, the Portuguese themselves were puzzled by the Indian languages; nor were the planters always certain of their slaves' origins. Phrases like “*pela lingua q. não he cristão*” (by his language not a Christian) indicate that the Portuguese were unsure of the linguistic stock of some slaves.⁶⁴ Despite these

⁶⁴ *DHA*, 3: 92. On Engenho Sergipe one of the field overseers, Tristão Pacheco, also served as translator (*lingoa do gentio*), a talent of benefit to the engenho. *DHA*, 3: 393-94. Portuguese and *mestiços*, lay and cleric,

differences, it is evident that the engenhos drew their Indian slaves from a wide range of geographical and cultural backgrounds. Engenho Sergipe counted among its slaves not only local Tupinambás, but peoples brought from Sergipe de El-Rey to the north, Rio das Contas to the south, and the *sertão* of the São Francisco River to the west, as well as a large contingent brought from Pernambuco. The inventories also include Carijos, Tamoios, and Cayetes, all Tupi-speakers and all from regions hundreds of miles from Bahia.

Nor were all the engenho Indians Tupians. The Sergipe and Santana inventories of the 1570s and 1591 often contain Tapuya names. Here and there other ethnic references appear—a few Tapanhuns (a Gê group), for example, and Nãmbipiras. Both inventories of the 1570s also contain many identifications that seem to reflect ethnic or tribal origins although they cannot be positively identified—Tinguas, Tarabes, Taipés, and the like. Engenhos Sergipe and Santana in particular, and probably most of the engenhos that depended on Indian labor, thus employed a heterogeneous Indian labor force. Whether this policy was intentional—that is, designed, as it later was for African slaves, to prevent their close cooperation and forestall rebellion—or was simply a response to the shortage of local laborers and market availability is moot. Northeastern planters were certainly willing to buy Indian slaves from other areas of Brazil and seemed to realize, as did the masters of Indian slaves elsewhere in the New World, that the effectiveness of Indian slavery increased when those enslaved could be moved from their native areas, making flight more difficult. Indians from São Paulo taken in the Jesuit missions of Paraguay reached Bahia and Pernambuco by sea, but their numbers were probably fewer than we have been led to believe. More common seems to have been the capture of Indians from the interior of the northeast.⁶⁵ Planters naturally preferred to acquire young male slaves.

who spoke Indian languages were proud of this accomplishment and usually pointed it out to the crown or other authorities since it was a necessary and valuable skill in the sixteenth and early seventeenth centuries. *Confissões da Bahia, 1591-92*, 87, 104-05, 167-72. Also see the petition of Luís de Aguiar, AGS, *Guerra antiga*, legajo 906.

⁶⁵ Such long-distance movements of Indian captives was common. The survivors of King Philip's War in New England were sent to Barbados, Carolina Indians went to New England and the Caribbean. Indians from northeastern Mexico were shipped to Hispanola, and an active slave trade sent Indians from Nicaragua to Peru, Panama, and the Caribbean. The list could be easily expanded. See Alden T. Vaughan, *New England Frontier: Puritans and Indians, 1620-1675* (Boston, 1965); Verner Crane, *The Southern Frontier, 1670-1732* (Ann Arbor, 1929), 113-15; Silvio Zavala, *Los indios esclavos en Nueva España* (Mexico, 1968); David Radell, "The Indian Slave Trade and Population of Nicaragua during the Sixteenth Century," in William Denevan, ed., *The Native Population of the Americas in 1492* (Madison, 1976), 67-76; and Jerome Handler, "The Amerindian Slave Population of Barbados in the Seventeenth and Early Eighteenth Centuries," *Caribbean Studies*, 8 (1960): 38-64. In their report to the crown of October 10, 1629, Simon Maceta and Justo Mancilla noted the arrival in Bahia of Indians from the south; see Jaime Cortesão, ed., *Jesuítas e bandeirantes no Guairá*, no. 1 of Biblioteca Nacional, Manuscritos da Coleção de Angelis (Rio de Janeiro, 1951), 310-39. The claims of authors such as Alfredo Ellis Junior, of large numbers of Indians being sent from São Paulo to the northeast in the seventeenth century seems greatly inflated given the paucity of documentation on the existence of an intercaptaincy trade. Moreover, the real shortage of African slaves in the northeast came after the Dutch seizure of Luanda in 1645, after the major slaving raids in the south had declined. See Ellis Junior, *Meio Século de Bandeirismo* (São Paulo, 1948), *passim*; and "O bandeirismo na economia do século xvii," in *Curso de bandeirologia* (São Paulo, 1956), 55-76. For the opposite interpretation, see Zenha, *Mamelucos*, 193-96.

TABLE 2
SEX DISTRIBUTION, ENGENHOS SERGIPE AND SANTANA, 1572-91

	<i>Married Males</i>	<i>Unmarried Males</i>	<i>Percent Males</i>	<i>Married Females</i>	<i>Unmarried Females</i>	<i>Percent Females</i>
Engenho Sergipe, 1572	51	41	61%	51	8	39%
Engenho Santana, 1572	18	47	60%	18	26	40%
Engenho Sergipe, 1591	17	19	58%	17	9	42%

The sex distribution of the Indian slave force was remarkably similar to that encountered later among black slaves. Usually about 60 percent of the slaves were male, and there was an understandable tendency for the men to be young adults. The marital status of Indian slaves and their ability to maintain family ties is difficult to determine from the Engenho Sergipe and Santana inventories because those records offer conflicting pictures. At Engenho Santana only eighteen married couples were listed in an adult population of one hundred and nine (sixty-five men and forty-four women). At Engenho Sergipe, closer to Salvador and perhaps to ecclesiastical observation, the percentage of married couples was much higher. Of the ninety-one men there, fifty-one were married. Whether this difference is explained by variant procedures for recording slave conjugal units or by a different composition of the slave force is impossible to establish. Perhaps more revealing is that among Engenho Sergipe's regular work force there were only eight unmarried women, and of these three were widows and two were related to other slaves at the mill. These figures, shown in Table 2, underline the expected preference of slave-owners for young males and suggest that little value was placed on female reproductive capacity.

Despite this preference, the nature of Indian slavery resulted in the presence of family units on the engenhos. Wives, children, siblings, or other relatives often accompanied the men into slavery. This pattern placed many people on or near the sugar mills and cane farms whose contribution to the process of sugar-making was marginal at best. In 1548 the great Schetz engenho in São Vicente had some one hundred and thirty slaves, half of whom were children or old people and thus of little use to the owners. Nevertheless, a contemporary observer called this slave force the best in the region.⁶⁶ The lists of Engenhos Sergipe and Santana also suggest a high ratio of semi- or unpro-

⁶⁶ Eddy Stols, "Um dos primeiros documentos sobre a engenho dos Schetz em São Vicente," *Revista de História*, 37 (1968): 407-20.

ductive slaves. At Engenho Santana some 25 percent of the total work force was too old, too young, or too sick to contribute very much to the mill's activity. Obviously, this percentage was greatly increased in the years of epidemic disease.

Women made up a significant part of the Indian labor force, but they were not as a rule considered to have skills that contributed to the making of sugar. Female slaves in the sixteenth-century inventories are invariably listed without occupation and their values resulted from combinations of age and health more than anything else. Thus, women appear as an omnipresent but not particularly valued sector of a plantation's operations. Some evidence from Engenho Sergipe suggests a recognition of the traditional role of Indian women in subsistence agriculture: a separate *roça* (farm) was maintained to supply the plantations' food needs, and a group of fifty slaves was assigned there. Two-thirds of these slaves were women—a proportion quite unlike the general sex ratio of the total population of the engenho.⁶⁷

Even when epidemic disease was not important, mortality rates were high. In 1572, a relatively plague-free year, five slaves died at Engenho Santana—a crude mortality rate of forty-three per thousand. In 1606 the chapel of Engenho Sergipe recorded thirty-two Indian deaths and only thirty-five baptisms. Because in this period accurate statistics for compiling general mortality rates are unavailable, there is some advantage in comparing figures with data from other regions of a similar social or economic composition. The crude mortality rate in Pernambuco in 1774 was almost thirty-three per thousand, and it remained at about that level until the late nineteenth century. In Maranhão the crude mortality rate for Indians in 1798 was close to twenty-two per thousand while that for black slaves was just over twenty-seven per thousand. Thus, the figure from Engenho Santana seems high, although it does not approach the rate of seventy per thousand experienced by African slaves in Jamaica and Barbados in the late seventeenth century.⁶⁸

For those Indians who survived and for their Portuguese masters and employers, the remaining major problems included introduction to the regime of large-scale export agriculture and adoption of cultural patterns acceptable to Portuguese religious and social sensitivities. The Portuguese did not permit the Indians to select those aspects of European culture that they found most suited to their needs but instead often forced them to adopt or accommodate those material and mental elements of culture on which the Portuguese placed a high priority. Even so, the process of Indian acculturation to the plantation work regime appears to have been slow and incomplete. Whatever kind of workers the planters hoped to create, elements of Indian culture reinforced at

⁶⁷ *DHA*, 3: 348–49.

⁶⁸ These figures are my calculations based on the census of Pernambuco in 1774 in *ABNR*, 40 (1918): 21–111; undoubtedly they reflect underregistration of high infant mortality of the eighteenth century. For the nineteenth century, see Peter Eisenberg, *The Sugar Industry of Pernambuco* (Berkeley and Los Angeles, 1974), 148–51; and Bainbridge Cowell, "Cityward Migration in the Nineteenth Century: The Case of Recife, Brasil," *Journal of Interamerican Studies and World Affairs*, 17 (1975): 43–63. For Maranhão, see BNL, *Fundo geral*, Códice 6936. And for African slaves in the Caribbean, see Michael Craton, *Sineus of Empire: A Short History of British Slavery* (New York, 1974), 194–95.

times by the “free” Indians who also worked at the mills, slowed the acculturation process.

The first superficial sign of acculturation was the adoption of a Portuguese name. The inventories of 1572–74 list many Indians who were still using their indigenous names exclusively, even though the Portuguese tended to assign easily recognizable and, for them, pronounceable names. Of the almost two hundred Indians at Engenho Sergipe, fifty still used only their original names. The inventory of Engenho Santana demonstrates that a period of transition existed during which the Portuguese used one name and the Indians another: entries such as “by her tongue Capea and by ours Domingas” or “Salvador, by his tongue Itacaraiba” frequently appear.⁶⁹ A comparison of the Engenho Sergipe inventories of 1572–74 and 1591 suggests gradual acculturation. Whereas the former contains some fifty slaves using only their Indian names, the latter has none. Thus, the Indians apparently began conforming to Portuguese patterns and, quite likely, newly captured peoples were relatively rare on the engenhos by the end of the century.⁷⁰ Their places were instead filled by free Indians, by Indians born in captivity and baptized with Christian names at that time, and by increasing numbers of African slaves.

The Indians’ eventual recognition and acceptance of their Portuguese names constituted steps toward their integration into the engenho community. Whenever possible, this process culminated in baptism of the formerly pagan Indians. The assumption of a new name was an important feature of Tupi social ascendancy that accompanied various events in life; among the Gê naming also served to recruit individuals for social and ceremonial roles. Thus, Indians could easily comprehend the significance of the baptismal ceremony and the relationship between a new name and a new status.⁷¹ Religion itself, in fact, provided a major avenue of acculturation. The participation of slaves in Church ritual and the sacraments, forced though that involvement may have been, provides a rough measure of the Indians’ integration into Portuguese society. For this reason, the chapel register of Engenho Sergipe is a valuable document, despite its incomplete and fragmentary condition. In its record of marriages (1600–26), burials (1598–1627), and especially baptisms (1595–1608) the basic patterns of sexual relationships and ritually defined responsibilities among the three principal racial groups are apparent. The period covered by the record is crucial, because between 1570 and 1630 the plantations crystallized into the distinctive social structure that characterized the area for the next two hundred years.⁷²

⁶⁹ DHA, 3: 348–49.

⁷⁰ Fernandes, *Organização social Tupinambá*, 178, 269, *passim*. Roberto da Matta presents evidence on naming among the Kraho and Apinaye; see “Notas sobre o contato e a extinção dos índios Gaviões do médio Rio Tocantins,” 189.

⁷¹ Inventory of 1591, ANTT, *Cart. Jesuitas*, maio 13, no. 4.

⁷² The chapel register is at present housed in the Arquivo da Curia Metropolitana of Salvador. It is bound and titled erroneously as the first register of baptism of Conceição da Praia. I consulted this register in 1968 and realized that it was not what its title indicated, but it was not until 1973 while working at the ACM again that David Smith recalled it to my attention and led me to examine it more thoroughly. Since the chapel, dedicated to Nossa Senhora da Purificação, served as the parish church for the region until a

The chapel register contains 234 complete adolescent baptisms for the period 1595 to 1608, or about 75 percent of the total baptismal entries. Of the 234 baptized, 171 (74 percent) were the children of slave mothers and, therefore, were slaves. The racial origins of the 134 mothers (for whom a racial designation has been determined) can be used as a rough gauge of the ethnic proportions of the population. It results in the following distribution: whites, 32 percent; Indians, 40 percent; Afro-Brazilians, 28 percent. Given the preference for males in the Atlantic slave trade, the sex ratio of Afro-Brazilians would be distorted and these figures thus underestimate this segment of the population. Still, Indians continued to be an important part of the plantations' population at the beginning of the seventeenth century—equaling, if not outnumbering, Africans and their descendants. The baptismal formula allows us to examine the relationship among four individuals—the father, mother, godfather, and godmother.⁷³ The patterns of these relationships reveal additional information about social organization and contacts during the period of Indian labor. Table 3 presents a distribution by ethnic/racial group of the four categories.

Baptism was somewhat less formal for slaves than it was for free people—especially for whites. Whereas a godmother and godfather were always present at the baptism of the child of a white couple, godparents were not always in attendance for slave children. There were some twenty-five instances (13 percent) when the godmother, the godfather, or both were omitted (or unregistered) at the ceremony. On one occasion a group of slaves served as godparents and on another there were two godfathers and no godmother. Such asymmetry and irregularity were never found among the Portuguese baptisms at Engenho Sergipe. When adult pagans were baptized, they usually received the sacrament in small groups of three or four. On these occasions one set of godparents might sponsor all of the baptized individuals.

According to Roman Catholic doctrine and practice, the roles of godfather and godmother were vital to the child's guidance. The relationship between child (*afilhado*) and godparents (*padrinhos*) was as binding as that between

new parish church was erected in Santo Amaro in 1722, the register contains information about the population of the whole surrounding parish and not solely of Engenho Sergipe. Unfortunately, there are drawbacks that limit the utility of the register for historical analysis. First, its present physical state is poor. Many entries are illegible because pieces of the pages are missing, and, in fact, most of the pages covering the sixteenth century have been lost, as have most of the marriage entries. The term "negro" was used to describe both Indians and Afro-Brazilians; thus, it is impossible to distinguish between them on this basis alone. Whites were never identified as such in the chapel register; therefore, when an individual has both a Christian name and family name and no other ethnic or color designation, I have assumed him to be white. This method probably results in a slight inflation of the white category at the expense of mulattoes and *mestiços*; but, since my major concern here is with Indians and Africans, this distortion is not serious. In addition, many individuals are simply described as "escravo" without more specific identification about color or origin. These problems complicate any analysis and the results presented here are tentative at best. Still, this singular source, an engenho chapel register from the late sixteenth and early seventeenth centuries, presents a rare opportunity to examine some aspects of relations between the various elements of the Recôncavo population.

⁷³ Each registration follows a formula: for example, "5 August Joana, young daughter of Tomé de Sousa, unmarried and of Luiza, an Indian of Domingos Ribeiro; godparents, Bras Dias and Antonia of the same Domingos Ribeiro." Chapel Register, Engenho Sergipe, 1595–1628, f. 96.

TABLE 3
RACIAL/ETHNIC DESIGNATIONS OF PARENTS AND GODPARENTS
ENGENHO SERGIPE, 1595-1608

	<i>White</i>	<i>Indian</i>	<i>African</i>	<i>Negro/ Crioulo</i>	<i>Mulatto</i>	<i>Unknown</i>
Father	61	42	27	6	0	98
Mother	43	54	33	8	3	93
Godfather	132	9	6	7	0	70
Godmother	59	21	8	7	7	114

NOTE: The many unknowns (98) in the case of the fathers result from illegitimate unions and no father present at baptism. In the cases of mothers, godfathers, and godmothers, unknowns are due to failure to report this information or gaps in the documentation.

TABLE 4
INDEX OF COMPADRIO PRESTIGE

	<i>Parents</i>	<i>Godparents</i>	<i>Ratio</i>
Whites	104	191	1.84
Indians	96	30	.31
Afro-Brazilians	64	13	.20

SOURCE: ACMS, Conceição da Praia (Engenho Sergipe) bautismos.

child and parents. The parallel set of ties between the parents and godparents established a set of mutual obligations and dependencies. The *padrinho* was the baptismal sponsor and usually assumed the expenses of the service. More importantly, the role of godfather placed very real obligations on those holding it, for it was not uncommon for godchildren eventually to depend on their padrinhos for economic assistance or protection. The position of the godfather had, therefore, status and prestige. Not surprisingly, a very high percentage of whites were godfathers in the baptisms examined. Not only did white parents always choose a white godfather, but Indians and Africans also sought whites to assume this role. In over 80 percent of the baptisms of the children of Indian mothers, white men served as godfather. Indian men served as godfathers in only nine cases; of the six cases in which the ethnic origins of the parents can be identified, all were Indian couples. African men were in a somewhat similar position and like the Indians continually looked to whites (or had to accept them) as godfathers for their children. Table 4 shows this white predominance.

The most pronounced pattern to emerge from the data concerning godparents is the marked difference between godfathers and godmothers: godmoth-

ers, it seems, were selected on a different basis from that of godfathers, for more Indian and Afro-Brazilian women were godparents than were men. White women rarely presented slave children for baptism. Instead, Indian mothers sought Indian godmothers. The godfather could be white for, as protector and benefactor, a white was best equipped to aid the child; the godmother, however, was an auxiliary to the child's upbringing and was a surrogate parent if the biological mother died. Although there were a few instances in which Indian women served as godmothers for children born to African women or in which mulatto women were godmothers to the children of Indian mothers, in the vast majority of the cases Indians, Africans, and whites selected women of the same racial category as *comadres*. Usually the godmother (*madrinha*) was a slave of the same master as either or both of the parents.

A few statistics illustrate this situation. Whereas male slaves comprised less than 12 percent of all godfathers registered, female slaves were over 30 percent of the godmothers. If only slave and *forro* baptisms are considered, the percentage of slave and *forro* *madrinhas* rises to more than 80 percent. The ties among the Indians were strong and in over 60 percent of the baptisms of children born to Indians, Indian women were godmothers. When both parents were Indians, 90 percent of the godmothers were also Indian. On occasion, a slaveowner, his relatives, or his employees stood as sponsor for a child of one of his slaves. But, significantly, planters did not often assume the godparent relationship—that is, it was not frequently used to reinforce the ties between slave and master. In less than 4 percent of the slave baptisms did the owner himself or a close immediate relative stand as sponsor. Patriarchalism may have existed, but it did not express itself very often through the role of *compadre*. Instead, what appears to be the dominant pattern is the selection of a white man who might intercede with the master in case of some future difficulty.

Despite this tendency toward endogamy, sexual relationships across the color lines did take place. White males were most easily able to take advantage of their dominant role in order to select sexual partners from among the slave and free populations. White men fathered over 11 percent of all children born to Indian mothers and 8 percent of those born to African women registered at Engenho Sergipe. If these figures are adjusted to include those cases in which no father was reported, a sign of illegitimacy and unstable or secret relationship, white men fathered 18.5 percent of all Indian children and almost 30 percent of the children of Afro-Brazilian women. Opportunities for association between Africans and Indians also existed in the slave quarters. The Engenho Sergipe inventories list a number of cases in which Africans and Indians formed permanent family units.⁷⁴ More impressive, however, is the relative lack of such unions in the chapel register, which contains only two cases of children born to such couples. Both of these were between Indian men

⁷⁴ Inventory, Engenho Sergipe, 1591, ANTT, *Cart. Jesuitas*, maço 13, no. 4.

TABLE 5
SLAVE MARRIAGES, ENGENHO SERGIPE, 1601-26

Indian/Indian	6
Same "Nation" African	7
Mixed "Nation" African	6
Crioulo/Crioulo	1
Unidentified Origins	9

and African women. In the period of transition from Indian to African slavery, therefore, the majority of Indians married or had sexual relations with other Indians and to a large extent remained sexually separate from those of other cultural backgrounds. What miscegenation that did take place occurred most frequently between whites and Indians or whites and Africans.⁷⁵

Among the three principal racial groups there was a strong tendency toward endogamy, at least in formal, Church-sanctioned unions. Between 1601 and 1626 in thirty slave marriages in which the origins of the partners could be determined, all were between individuals of the same racial group, although not of the same ethnic or linguistic group.

THE TRANSITION FROM A PREDOMINANTLY INDIGENOUS SLAVE FORCE to one composed mainly of Africans occurred gradually over the course of approximately half a century. As individual engenho owners acquired sufficient financial resources, they bought a few African slaves and they added more as capital and credit became available. By the end of the sixteenth century, engenho labor forces were racially mixed and the proportion increasingly changed in favor of imported Africans and their offspring. In the 1550s and 1560s there were virtually no African slaves at the northeastern sugar mills.⁷⁶ By the mid-1580s Pernambuco had some sixty-six engenhos and a reported two thousand African slaves. If we estimate an average of one hundred slaves per engenho, then Africans composed one-third of the captaincy's slaves.

In Bahia, the change can be observed in the transformation of a single engenho's population over time. In 1572 Engenho Sergipe had two hundred and eighty adult slaves of which only twenty (7 percent) were African. Twenty years later, in 1591 the engenho had a slave population of one hundred and three, of which thirty-eight (37 percent) came from Africa. When, in 1638, Engenho Sergipe was rented to Pedro Gonçalves de Mattos, it had eighty-one slaves, all of whom were African or Afro-Brazilian.⁷⁷ The transition to an

⁷⁵ This pattern continued throughout the colonial period in Bahia. In the parish of Inhambupe between 1750 and 1800, 80 percent of the 1,294 registered marriages were between couples of the same racial category. See Conselho Pôndé de Sena, "Relações interétnicas através de casamentos realizados na freguesia do Inhambupe, na segunda metade do século xviii," unpublished paper, Universidade Federal da Bahia (Salvador, 1974).

⁷⁶ Mauro, *Le Portugal et l'Atlantique au XVII^e siècle* (Paris, 1960), 192-94. The Jesuits of Bahia asked for two dozen Africans in 1558, "and these can come together with those the king sent to the [royal] engenho because often he sends ships here loaded with them." Leite, *Cartas Nobrega* (Bahia; May 8, 1558), 288.

⁷⁷ Inventário de Mem de Sá, 1472, in *DHA*, 3: *passim*; Engenho Santana had the same distribution with 7

African labor force was made in the first two decades of the seventeenth century at a time when the sugar industry experienced rapid expansion and considerable internal growth arising from high international sugar prices, a growing European market, and, perhaps, the peaceful maritime conditions brought about by the twelve-year truce between Spain and the Netherlands (1609–21). A comparison of the positions and roles of Indian and African slaves should help explain why the transition to African labor took place.

The shift to African labor depended in part on Portuguese perceptions of the relative abilities of Africans and Indians. With a long history of black slavery in Iberia which had intensified during the expansion of the sugar industry in the Atlantic, the Portuguese were well acquainted with Africans and their skills. By the end of the sixteenth century Africans had already impressed the Portuguese with their ability to master the techniques of sugar production on Madeira and São Tomé. The Portuguese in Brazil, long familiar with the use of blacks as servants, urban artisans, and skilled slaves in Portugal and the Atlantic islands, began to look toward Africa as a logical source for these skills. The first black slaves in Brazil came as body servants or skilled laborers, not as field hands. The three extant engenho slave lists from the sixteenth century indicate a high percentage of Africans with various skills, and invariably the most complicated tasks assigned to slaves were given to Africans. In 1548 Engenho São Jorge de Erasmus in São Vicente had about one hundred and thirty slaves “of the land” as well as seven or eight Africans. All of the Africans were *oficiais*, that is skilled at various tasks, and one was sugar master, the most important managerial position on any engenho. The director of Engenho São Jorge’s operations proudly wrote to the absentee owners, the Schetz family of Antwerp, that sugar masters on Madeira usually received thirty milréis a year, a sum which their engenho now saved by using this skilled black slave.⁷⁸ Three other Africans were employed in positions requiring skilled judgment, one as purger (*purgador*) and two as kettleman (*caldereiro*).

A similar situation is found in the inventory lists of Engenhos Sergipe and Santana of the late sixteenth century. At Engenho Sergipe Indians and Africans were used in different ways during the period of transition. Because the engenho could afford Portuguese technicians and managers, the occupational pyramid of the slaves was truncated. The work force was heavily indigenous, out of 134 male slaves, 115 were Indians. The same proportion of Africans and Indians were listed with a specific occupation; but, when certain jobs such as fishermen, hunters, boatmen, are not included, the proportion of Indians with special occupations drops considerably. Table 6 shows these differences.

Africans in a slave force of 107 or 6.3 percent. Inventory, Engenho Sergipe, 1591. ANTT, *Cart. Jesuitas*, maço 13, no. 4. This listing of slaves was made in a legal dispute between Francisco de Negreiros, representative of the count of Linhares, and Francisco Araújo and his wife, who claimed to have purchased the engenho in 1590. “Treslado do Inventário do Engenho Sergipe,” 1638, ANTT, *Cart. Jesuitas*, maço 30, f. 1040.

⁷⁸ Stols, “Um dos primeiros documentos sobre a engenho dos Schetz,” 418–20.

TABLE 6
OCCUPATIONAL STRUCTURE
ENGENHO SERGIPE 1572, 1591

	1572		1591	
	<i>Africans</i>	<i>Indians</i>	<i>Africans</i>	<i>Indians</i>
Sugar-making skills				
mestre de açúcar (sugar master)			1	
ajuda do mestre (assistant)			1	
purgador (purger)			2	1
ajuda do purgador (assistant)	1	2	1	
tacherio (small kettleman)	1	2	3	
escumeiro (skimmer)	1			
ajuda do escumeiro (assistant)		3		
caldereiro (kettleman)		6		
moedor (mill tender)		3	2	
premseiro (presser)	1	1		
virador de bagaço (bagasse feeder)		1	1	
caixeiro (crater)		2	1	1
dos melles (molasses)	1	1		
Artisan skills				
carapina (carpenter)		1		
ferreiro (blacksmith)			1	
calafate (boatcaulker)		1		
falleiro (?)	1	1		
Auxilliary skills				
vaqueiro (cowboy)	1	1	2	1
carreiro (carter)	1	1		
boieiro (herdsman)				3
pescador (fisherman)		11		
serrador (sawyer)		7		
lenadeiro (firewood)				1
porqueiro/ovelheiro (pig/sheep tender)		2		
“barcas” (boatman)	1	4		
Management				
feitor (overseer)		1		
Total	9/19	51/115	15/30	7/65

The inventory of 1572—taken when Indians were still plentiful and relatively inexpensive to obtain, when Africans were still not available in large numbers, and when the legislation against Indian slavery was not yet effectively in force—represents a specific period in the history of Indian slavery. Twenty years later the situation had changed considerably. By 1591 the sugar economy of the northeast was expanding rapidly to supply a growing European demand. The Atlantic slave trade had been regularized to the extent that the supply, while not yet great, was at least dependable. The majority of Engenho Sergipe's slave force was still Indian, but Africans and Afro-Brazilians now filled almost all of the skilled occupations on the estate. Angolan and Guinean men were employed as sugar master, purger, assistant purger, blacksmith, kettleman, and sugar crater. Others were employed in the milling operations of the engenho and a few as cowhands. The Indian occupations were far more rudimentary and, aside from one sugar crater, only three were listed with occupations, one woodcutter and two herdsmen. In other words, when possible, the Portuguese turned to Africans to provide skilled slave labor.

This policy, like the relative price of Indians and Africans, is to some extent explained by demographic and cultural features of both peoples. Many West Africans came from cultures where iron working, cattleherding, and other activities of value to sugar agriculture were practiced.⁷⁹ These skills and a familiarity with long-term agriculture made them more valuable to the Portuguese for the specific slavery of sugar. Africans were certainly no more "predisposed" to slavery than were Indians, Portuguese, Englishmen, or any other people taken from their homes and bent to the will of others by force, but the similarity of their cultural heritage to European traditions made them more valuable in European eyes. The Indian susceptibility to European disease at all ages made riskier the investment of time and capital in training them as artisans or managers. Africans, of course, also suffered in the environment of Brazil, but the highest rate of black mortality was always found among the newly arrived (*boçal*) and among infants. Thus, once a slave was "seasoned" and had passed through infancy and childhood, the chances of survival and therefore of safe investment in skill were very good.

African health and skill, as well as lack of resistance, may explain the reluctance of planters to invest in the training of Indian slaves, but it does not respond to the question why, even in cases where Indians were free workers earning a wage, the value of their labor was considered unequal to that of whites, mulattoes, and free blacks. At Engenho Sergipe an Indian carpenter received only 20 percent of the wages paid to whites for the same task. During the seventeenth century Indian workers received only twenty réis a day and

⁷⁹ The Portuguese slave trade in the sixteenth century was concentrated in the Senegambia. On the cultural and agricultural traditions of the peoples in that region, see Phillip D. Curtin, *Economic Change in Precolonial Africa: Senegambia in the Era of the Slave Trade* (Madison, 1975), 3–58; and Walter Rodney, *A History of the Upper Guinea Coast, 1545–1800* (Oxford, 1970), 1–38.

skilled artisans averaged thirty réis. In the 1630s the municipal council of Salvador paid Indian laborers a daily wage of thirty réis and Paraíba Indians could be paid in manioc and cloth a daily wage of about fifteen réis. Black slaves, by contrast, could earn an average of 240 réis per day.⁸⁰

The wage labor system, therefore, constantly proposed as the ideal way to integrate the Indian into colonial society, was a failure. Indians were often reluctant to participate in the labor market; and the Portuguese, furthermore, did not really allow that market to operate freely: the wages paid to Indians were always below existing rates.⁸¹ The colonists placed Indian wage earners on a scale of reward and labor different from that of other workers. At Engenho Sergipe they were usually paid by the month rather than by the day or, even more commonly by the task. Their work did not usually require completion at a specific time, and often they received payment in kind rather than in cash. Manioc flour, trade cloth (*pano*), and alcohol were the common "wages" for Indians from Maranhão to São Paulo.⁸² Obviously, the Portuguese seemed to believe, for whatever reasons, that Indian workers could not be treated like others.

There was, in fact, a remarkable similarity among all of the colonial regimes in the New World in the low value placed on Indian laborers in comparison with Africans. In times and places as widely different as sixteenth-century Mexico, seventeenth-century Brazil, and eighteenth-century Carolina, Spaniards, Portuguese, and Englishmen held similar opinions of Indian and African laborers. The colonists in each situation usually valued Africans three to five times higher than the Indians.⁸³ Certainly, market availability, demographic patterns, opportunities for flight or resistance (management costs), and European prejudices entered into these calculations. Still, despite the racist implications of arguments about the relative adaptability of one people over another to tropical labor, the similarity of opinion among all the New World slaveholding regimes suggests that there was a comparative advantage, especially in the formative period of slaveholding, in the use of African rather

⁸⁰ Paul Silberstein, "Wage Earners in a Slave Economy"; and Biblioteca Nacional de Madrid, Códice 2436, ff. 105-09.

⁸¹ See Antonio García, "Regímenes indígenas de Salariado: El Salariado natural y el Salariado capitalista en la historia de América," *América Indígena*, 8 (1948): 250-87.

⁸² Silberstein, "Wage Earners," based on *DHA*, 2: *passim*; and Leite, *HCJB*, 2: 63. Also see Adrien van der Dussen, *Relatório sobre as capitanias conquistadas no Brasil pelos holandeses*, ed. José Antônio Gonçalves de Mello, (Rio de Janeiro, 1947), 88-89.

⁸³ Almon Wheeler Lauber, *Indian Slavery in Colonial Times within the Present Limits of the United States*, Columbia University Studies in History (New York, 1913), 298-300, presents scattered references to relative prices from New England, New York, and the Carolinas. Crane, *The Southern Frontier, 1670-1732*, 113-15, provides data showing that Indians were valued at one-half to one-third the price of black slaves. Peter H. Wood, *Black Majority* (New York, 1974), 38-40, reviews the literature on Indian slavery in Carolina but is silent on this point. Instead see John Donald Duncan, "Servitude and Slavery in Colonial South Carolina, 1670-1776," 2 vols. (Ph.D. dissertation, Emory University, 1972). On French Canada, see Marcel Trudel, *L'esclavage au Canada Français: Histoire et condition de l'esclavage* (Quebec, 1960); and Guy Fregault, *La civilisation de la Nouvelle-France* (Montreal, 1944), 83-84. Colin Palmer, *Slaves of the White God: Blacks in Mexico, 1570-1650* (Cambridge, Mass., 1976), 34, provides considerable evidence from the 1520s in Mexico as does Zavala, *Los indios esclavos en Nueva España*. Most important is Gonzalo Aguirre Beltrán, "El trabajo del indio comparado con el del negro en Nueva España," *México Agrario*, 4 (1942): 203-07.

than Indian slaves and that this advantage was based on productivity in terms of return on investment. The statement of one observer in the Carolinas in 1740 that “with them [Indians] one cannot accomplish as much as with Negroes” was echoed everywhere in the Americas.⁸⁴

In Brazil, the relative position of Indian and African slaves within the sugar labor force can be seen in its simplest and crudest form in the comparative prices of the two peoples. The average price of an African slave listed with occupation in 1572 was 25\$000 while Indians with the same skills averaged only 9\$000. The only skilled Indians whose price equaled, or even approached, that of African slaves were those who were truly practicing skilled crafts—carpenters, sugar craters, and boat caulkers, for example, or those engaged in the specialized positions of a sugar mill. The vast majority of Indians listed with some occupation, but not an artisan skill, were priced far below the average value of unskilled Africans. The price difference between skilled and unskilled Indians was greater, moreover, than that among Africans.

There is evidence that these values represent real differences in the productivity of Indian and African labor. Production figures from Bahia at the close of the sixteenth century support this interpretation. Although there is some discrepancy in the reported total of *engenhos*, a number of accounts list fifty mills operating in the captaincy of Bahia by 1590.⁸⁵ For the year 1589 Father Francisco Soares reported that there were fifty *engenhos*, eighteen thousand slaves, and thirty-six thousand *aldeia* Indians.⁸⁶ If we assume that two-thirds of the slave force were involved in sugar agriculture, then the ratio of slaves to *engenhos* was 240 to 1. This figure—which does not include any of the settled Indians that also provided labor to the *engenhos*—is extremely high. It represents not only slaves owned directly by the mills, but those owned by tenants, sharecroppers, and others as well. Father Soares estimated an annual production per mill of four thousand *arrobas* or fifty-eight tons. Thus, each slave produced at the time almost seventeen *arrobas* (over five hundred pounds) a year—a very low level of productivity, since the later calculation in Brazil based on black slaves was forty to seventy *arrobas* annually.⁸⁷ Even allowing for technological changes and inexact information, the only conclusion that can be drawn from such figures is that Indian labor was characterized by low

⁸⁴ As quoted in Duncan, “Servitude and Slavery in Colonial South Carolina, 1670–1776,” 36. For the traditional racist arguments of the nineteenth century, see Herman Merivale, *Lectures on Colonialism and Colonies* [1861] (London, 1967), 283.

⁸⁵ Gabriel Soares de Sousa lists thirty-six *engenhos* for Bahia, but he also speaks of eight *casas de melles* (molasses-producing units). He gives an annual production of 120,000 *arrobas* for the captaincy or somewhat less than 4,000 *arrobas* per mill. Fernão Cardim also speaks of thirty-six *engenhos* in Bahia, but José de Anchieta lists forty-six. I have taken Father Soares’ figure of fifty because it yields the lowest ratio of slaves to *engenhos* as a control on my argument that the ratio is extraordinarily high. Using the estimates of Soares de Sousa or Cardim yields over 333 slaves for each mill; see Mauro, *Le Portugal et l’Atlantique*, 193; and Mauricio Goulart, *Escravidão africana no Brasil* (São Paulo, 1950), 100.

⁸⁶ Soares, *Coisas notáveis do Brasil*, 11.

⁸⁷ See Ward Barrett and Stuart B. Schwartz, “Two Colonial Sugar Economies: Bahia and Morelos, 1600–1800,” in Enrique Florescano, ed., *Haciendas Latifundias y Plantaciones* (Mexico, 1975), 550–55. Also see Ward Barrett, *The Sugar Hacienda of the Marqueses del Valle* (Minneapolis, 1970), 98–99.

productivity. As Magalhães de Gandavo claimed, “if the Indians were not so fickle and given to flight, the wealth of Brazil would be incomparable.”⁸⁸

At the time Father Soares made his estimates, Bahia had between three and four thousand African slaves; thus, three-fourths of its slave force was still Indian in the last decade of the sixteenth century.⁸⁹ With the low level of Indian productivity, the price differential between African and Indian slaves becomes readily understandable. In the inventories of 1572–74 African slaves were valued at an average price of 20\$000, while adult Indians averaged about 7\$000.⁹⁰ This ratio of roughly three to one is also the ratio between the estimates of African and Indian productivity in sugar agriculture. It would appear that the Portuguese made a reasonable economic calculation of the comparative profitability of their two alternate work forces. Africans surely cost more to obtain, but in the long run they were a more profitable investment.

WE CAN SPECULATE THAT THE EARLY PRESENCE of large numbers of Indians allowed the mills to begin production with a small original capital outlay in slaves. The expansion of the sugar economy in the 1550s and 1560s depended on the availability of this source of “cheap” labor. During the 1570s, however, resistance, plague, and anti-enslavement legislation reduced the availability—and profitability—of Indians. Plantation owners now found that the cost differential between Indian and African laborers no longer outweighed differences in productivity between the two labor forces. This disparity in overall productivity also helps to explain why the Portuguese preferred imported Africans to coerced but “free” Indians. While there were occasional proponents of *encomienda* or of peonage in Brazil, the colonists believed that, given the high mortality and low productivity of Indians, Africans were a better investment. The Bahian historian and planter, Sebastião da Rocha Pitta, probably summarized majority opinion when he observed that Indians suffered from “working by obligation rather than out of desire, as they had in the state of freedom; and in its loss and in their repugnance to, and thought of, captivity so many die that even at the lowest price they are expensive.”⁹¹

A discussion of profitability in strictly neoclassical economic terms will not suffice as an explanation of the transition of the labor force. There were always cultural and political determinants as well. Not everyone in Brazil was convinced of the wisdom of the shift. Portuguese colonists were generally unwilling to surrender control of Indians, especially when they could be obtained for nothing. The colonists demonstrated their reluctance by political remonstrance and demonstration—most notably in 1609 and 1640. Gaspar da Cunha, overseer of Engenho Sergipe, wrote to the Count of Linhares in 1585 that Africans “cost too much and are prejudicial to the plantation and to the

⁸⁸ Gandavo, *Histories of Brazil*, 153.

⁸⁹ Goulart, *Escravidão africana no Brasil*, 100.

⁹⁰ Buescu, *300 anos da inflação*, 44–45.

⁹¹ Sebastião da Rocha Pitta, *História da America portuguesa* (1730; 2d ed., Lisbon, 1880), 196–97.

neighborhood; they are neither as necessary nor as beneficial as the Indians of this land."⁹² He then petitioned for more free Indians to be brought to the *engenho*. By the early seventeenth century such requests and sentiments were far less frequent. The shift to African labor was well on its way, especially in the northeastern sugar region where capital had accumulated and the patterns of international commerce were securely established. Colonial slavery had emerged as the dominant mode of production, and the process of its emergence was not dictated by the market so much as by the organization of production. The system of labor and nature of the labor force was determined not only in the court at Lisbon or in the counting houses of Amsterdam and London but in the forests and canefields of America.

⁹² Gaspar da Cunha to the count of Linhares (Bahia; August 28, 1585), ANTT. *Cart. Jesuitas*, maço 8, no. 9. For a similar opinion see Martim Leitão's *Parecer*, in which he valued one Indian equal to four Guiné slaves. BA. 44-XIV-6, ff. 185-93v.

Obstacles to Economic Growth in Nineteenth-Century Mexico

JOHN H. COATSWORTH

THE ECONOMIC HISTORY OF MEXICO HAS ADVANCED dramatically in recent years. The most important revisionist work has concentrated on the eighteenth and nineteenth centuries, although major works have appeared on the sixteenth, seventeenth, and twentieth centuries as well.¹ Much of the new research has been quantitative or comparative, linking the study of Mexico's past to methodological currents in the United States and Western Europe.² It is now possible to revise some of the principal hypotheses about Mexico's relative backwardness since the end of the colonial era. Comparative estimates of Mexican gross national product in the nineteenth century can be used to assess the impact of revisionist work on standard interpretations of Mexico's economic history from the Bourbons to the Revolution of 1910.

By way of introduction, two caveats are worth stressing. First, spurious precision infects all, or nearly all, quantitative historical work. Margins of error are necessarily large in estimates of national product based on fragmentary data, the guesswork of interested contemporaries, and the imagination of the modern researcher. This is even truer of estimates produced to test hypotheses about what might have been, as the "new economic history" has amply demonstrated during the past decade or so in the United States. I take it as a duty, therefore, to issue a clear warning: all of the numbers in this article are, without exception, inaccurate; however, that is not a valid argument against their use. Literary estimates typically contain fewer errors (we

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¹ For the colonial period, see Charles Gibson's excellent review, "Writings on Colonial Mexico," *Hispanic American Historical Review* (hereafter *HAHR*), 55 (1975): 287-323. For a review of the nineteenth- and twentieth-century work, see Enrique Florescano, "Ensayo de Interpretación," Introduction to the section on Mexico in Roberto Cortés Conde and Stanley J. Stein, eds., *Latin America: A Guide to Economic History, 1830-1930* (Berkeley and Los Angeles, 1977), 435-55.

² See the many references to Mexico in John J. TePaske, "Recent Trends in Quantitative History: Colonial Latin America," *Latin American Research Review*, 10 (1975): 51-62. TePaske's own work on colonial fiscal data provides an excellent example of the new trends: *La real hacienda de Nueva España: La real caja de México (1576-1816)*, no. 41 of *Colección científica: Fuentes*, Instituto Nacional de Antropología e Historia (Mexico, 1976). TePaske's Mexican study forms part of a larger project, undertaken in collaboration with Herbert Klein, that involves the reconstruction of treasury office accounts in all of the major colonial centers.

all know that Mexico was “poorer” than its northern neighbor), but only because they specify a ridiculously wide range of values. Numerical estimates have the virtue of inspiring controversy and thus may lead to the compilation of more accurate statistics. As Charles Gibson reminds us, even failures can prove fruitful for the advancement of historical knowledge.

Second, the precision of historiographical essays can be equally spurious. Precision requires restatement, the deliberate partitioning of what can be rejected from what was really intended. To set aside a well-defined error is easier than to modify a complex interpretation that rests on many legs at once. Separating what makes sense from what does not inevitably does violence to interpretations that contain elements of both or that rest on analytical distinctions the reviewer does not find important.

ESTIMATES OF MEXICAN NATIONAL INCOME reveal expected trends. Per capita as well as total income fell until sometime after 1860. A recovery began during the Restored Republic (1867–76), but Mexico did not surpass colonial income levels until well into the Porfiriato (1877–1910). Between 1877 and 1910 national income per capita grew at an annual average rate of 2.3 percent—extremely rapid growth by world standards, so fast indeed that per capita income more than doubled in thirty-three years.

In Table 1, I have assembled estimates of national income for Mexico, Brazil, Great Britain, and the United States at selected years from 1800 to 1910. (These are the only countries for which such estimates are available for the entire nineteenth century.) The comparative data show that national income per capita in Mexico was closer to that of Great Britain and the United States in 1800 than at any point thereafter. In that year Mexico produced more than a third of British income per head and nearly half that of the United States. The gap in productivity between the Mexican economy and that of the advanced countries of the North Atlantic has never been so small. By 1877, Mexico’s per capita income had fallen to a little over one-tenth that of the industrial nations. It has fluctuated between 10 and 15 percent of U.S. per capita income ever since. The comparison with Brazil shows a different pattern. At the beginning of the century, Mexico’s per capita income was nearly 20 percent above that of Brazil. While Mexican productivity fell during most of the century, Brazil’s increased and surpassed that of Mexico. During the Porfirian period, however, the Mexican economy grew very rapidly and by 1910 Mexico’s per capita income was 40 percent higher than Brazil’s. Today, the difference between these two countries stands about where it was in 1800.³

In terms of total size, the contrast between Mexico and the industrial

³ The data in Table 1 may be compared to estimates of per capita income for other Latin American countries cited in William P. McGreevey, “Recent Research on the Economic History of Latin America,” *Latin American Research Review*, 3 (1968): 98–99. Also see Laura Randall, *A Comparative Economic History of Latin America, 1500–1914*, 1 (Ann Arbor, 1977): 224, for an attempt to construct “upper limit” estimates of Mexican national income beginning in 1520.

TABLE 1.
NATIONAL INCOMES, 1800-1910

A. PER CAPITA INCOME				
YEAR	<i>Mexico</i>	<i>Brazil</i>	<i>Great Britain</i>	<i>United States</i>
1800	73	62 (118)	196 (37)	165 (44)
1845	56	72 (78)	323 (17)	274 (20)
1860	49	77 (64)	370 (13)	359 (14)
1877	62	83 (75)	497 (12)	430 (14)
1895	91	89 (102)	745 (12)	735 (12)
1910	132	94 (140)	807 (16)	1035 (13)

B. TOTAL INCOME (MILLIONS)				
YEAR	<i>Mexico</i>	<i>Brazil</i>	<i>Great Britain</i>	<i>United States</i>
1800	438	198 (221)	2094 (21)	858 (51)
1845	420	510 (82)	6293 (7)	5493 (8)
1860	392	778 (50)	8510 (5)	10900 (4)
1877	613	1115 (55)	16690 (4)	21629 (3)
1895	1146	1633 (70)	27930 (4)	50754 (2)
1910	2006	2129 (94)	36556 (5)	95201 (2)

NOTE: Parenthetical numbers show Mexican income as a percent of each estimate. The estimates have all been computed in U.S. dollars of 1950.

SOURCES: Data and estimating procedures for Mexican national income available from author on request. Note that the 1845 and 1860 figures for Mexico represent the lower of two alternative estimates constructed for these years. In both cases, the alternative estimates are 5 percent higher than those in the table. For Brazil, see Nathaniel H. Leff, "A Technique for Estimating Income Trends from Currency Data and an Application to Brazil," *Review of Income and Wealth*, 5 (1972): 335-68. Leff calculated that Brazil's national income grew at an annual average rate of 0.4 percent between 1822 and 1913. Values in the table are extrapolated back from an estimated U.S. \$98 in 1920-25 at this rate. For Great Britain, see Phyllis Deane and W. A. Cole, *British Economic Growth, 1688-1959: Trends and Structure* (Cambridge, 1962), 282, 329-30. The initial estimate for 1800 employs the Deane and Cole figure of £232 for 1801, converted to 1950 dollars by using the Warren Pearson and Bureau of Labor Statistics (WP/BLS) wholesale price index in U.S. Bureau of the Census, *Historical Statistics of the United States from Colonial Times to 1957* (Washington, 1958), 115-17. Subsequent British estimates employ the Deane and Cole growth rates applied to the initial figure. The estimates for 1800, 1845, and 1860 are for Great Britain alone; the rest are for the United Kingdom (which includes Ireland). The U.S. estimates for 1800, 1845, and 1860 are based on Paul A. David, "The Growth of Real Product in the United States before 1840: New Evidence. Controlled Conjectures," *Journal of Economic History*, 27 (1967): 151-97. David's estimates of real growth rates between 1800 and 1860 were used to extrapolate his estimate of U.S. gross national product in 1840, adjusted to 1950 dollars with the WP/BLS index. For 1877, 1895, and 1910 a similar adjustment to 1950 dollars was made in the national income estimates in U.S. Bureau of the Census, *Historical Statistics*, 139. Note that the 1877 figure is an average estimated for 1877-81 and the 1895 estimate is an average for 1892-96.

nations, especially the United States, is even sharper. In 1800, Mexico produced more than half as many goods and services as the United States. By 1877, Mexico produced only 2 percent of the output that flowed from the factories, farms, and warehouses of the northern colossus and only 5 percent of total British output. Population growth accounts for a large portion of the difference between Mexico and the two industrial economies. In 1800, Mexico's population of six million was larger than that of the United States (just over five million) and over half that of Great Britain (almost eleven million, excluding Ireland). In 1910, Mexico's population stood at fifteen million, the United Kingdom at forty-five million, and the United States at ninety-two million. Even Brazil, whose population in 1800 was only somewhat more than three million, passed Mexico during the nineteenth century and reached over twenty-two million by 1910. Both the United States and Brazil received large numbers of immigrants, especially after 1890. Mexico attracted very few immigrants, while high death rates kept down the natural increase in population until the 1930s.⁴

Although the estimates in Table 1 may contain errors, the relative position of the four countries and the magnitude of the gaps among them are not likely to be affected by better data. For an analysis of Mexico's relative backwardness in the nineteenth century, two aspects of the comparison stand out. The first is the substantial difference between the economies of Mexico and the United States at the beginning of the century. In 1800 the United States was still a predominantly agrarian country, decades away from its industrial revolution. Mexico in the same year counted as Spain's richest colony in the New World, with an advanced mining industry exporting vast quantities of processed metals. Indeed, the values of U.S. and Mexican exports at the end of the eighteenth century were quite similar.⁵ Why did the Mexican economy begin the nineteenth century less than half as productive as that of the United States? The second striking aspect of the data is the extent to which the gap between Mexico and the industrializing countries widened between 1800 and the last quarter of the century. Had Mexico's economy kept pace with the growth of the United States for the entire century, Mexico would have reached its 1950 level of per capita income before the Revolution of 1910. Had the gap between Mexico and the United States remained the same from 1800 to the present, Mexico would now rank among the world's industrial powers. From the standpoint of the twentieth century, the question might be reversed: Why did the Mexican economy fall so far behind the industrializing giants of the North Atlantic during the nineteenth century?

⁴ The 1910 British estimate includes Ireland. For population data for these countries, see the sources cited in Table 1. For Mexico, see El Colegio de México, *Estadísticas económicas del Porfiriato: Fuerza de trabajo y actividad por sectores* (Mexico, n.d.), 1.

⁵ The United States exported roughly twenty million pesos (or dollars; the exchange rate was 1:1) worth of goods annually just prior to the Napoleonic Wars. *American State Papers*, 7, Class 4: *Commerce and Navigation* (Washington, 1832), 1: 34ff. Mexican foreign trade data begin in 1796. While U.S. trade increased markedly during the war, Mexico's did not. Miguel Lerdo de Tejada, *Comercio exterior de México desde la Conquista hasta hoy* (1853; reprint ed., Mexico, 1967), no pagination.

TABLE 2
COMPARISON OF THE COSTS OF BRITISH AND SPANISH MERCANTILISM

COLONY	<i>Costs (millions of pesos)</i>	<i>Costs Per Capita (pesos)</i>	<i>Costs as Percent of Colonial Income</i>
BRITISH NORTH AMERICA (1775)	0.5	0.26	0.3
NEW SPAIN (1797-1820)			
Trade Burden	7.2	1.20	3.0
Fiscal Burden	10.1	1.68	4.2
TOTAL	17.3	2.88	7.2

NOTE: Colonial income for the thirteen British colonies is calculated as the 1800 per capita estimate (U.S. \$90) multiplied by the 1775 population. In current pesos of 1800, this comes to a total income of 173 million pesos. Note, however, that Thomas estimated the costs of British mercantilism in current dollars of 1775, before the inflation that began in the 1780s. Thus, the magnitude of the British burden as a percent of total and per capita income is somewhat overstated. For New Spain, the estimates are annual averages for 1797 to 1820.

SOURCES: For British North America, see Robert Paul Thomas, "A Quantitative Approach to the Study of the Effects of British Imperial Policy upon Colonial Welfare," *Journal of Economic History*, 25 (1965): 615-38. For Mexican estimates, see text and notes 7-8.

THREE MAIN OBSTACLES TO ECONOMIC GROWTH have been postulated to explain Mexico's relative backwardness at the end of the colonial period: Spanish colonial rule, the system of land tenure, and the Roman Catholic Church. On the basis of evidence of direct economic effects, regardless of more general problems, all must be rejected.

First, since Spain managed, or mismanaged, its New World colonies for three full centuries, it cannot be denied that Spain was responsible for whatever the colonies achieved or failed to achieve—a truism that has been repeated by scholars and historians ever since Baron Alexander von Humboldt made it popular in his monumental studies of the Spanish colonies in the first decade of the nineteenth century. But emancipation from Spain promised few benefits. For purposes of measurement, the costs of Spanish colonialism are defined as those economic constraints that independence actually eliminated: (1) mercantilist restrictions on direct trade with foreign countries and (2) the uncompensated export of gold and silver extracted by the colonial government as net fiscal revenues. (This definition leaves for later discussion those effects of Spanish colonialism that lingered on after independence.) There are two questions for which precise answers may be sought. How much would the Mexican economy have gained if independence from

Spain had been achieved by the end of the eighteenth century? And how much of the gap in productivity between the U.S. and Mexican economies would have been eliminated by these gains?

Table 2 presents estimates of the answers. It compares the annual average cost of Spanish colonialism to Mexico (then called New Spain) between 1797 and 1820 with the cost of British colonialism to the thirteen North American colonies in 1775. The North American figures are taken from Robert Paul Thomas' well-known estimates,⁶ which calculate the loss of colonial income due to British restrictions on direct trade (mainly with northern Europe and the foreign West Indies) and subtract from this loss certain benefits (bounties for the production of strategic materials, naval and frontier protection, and the like). The Mexican estimates have two components. The first is an estimate of colonial income lost through Spanish restrictions on direct foreign trade, similar to Thomas' estimates for the British colonies.⁷ The second component is the fiscal burden—tax revenues collected by crown officials in Mexico and exported to subsidize Spanish administration of other colonies or to fill the king's coffers at Madrid.⁸ Since the British raised no internal taxes, no such burden existed in the thirteen colonies.

As Table 2 indicates, the total cost of Spanish sovereignty came to a little more than seventeen million pesos per year. By contrast, the cost of British colonialism was only half a million pesos. The Spanish burden was, therefore, thirty-five times greater than that of the British. Table 2 also shows, however, that the Spanish burden amounted to less than three pesos per capita at the beginning of the nineteenth century. It is arguable, of course, whether the psychic splendors of Spanish citizenship were worth that much. In economic terms, fully 7.2 percent of the colony's 1800 income was lost, an amount by no

⁶ Thomas, "A Quantitative Approach to the Study of the Effects of British Imperial Policy upon Colonial Welfare," *Journal of Economic History*, 25 (1965): 615–38. Thomas has been criticized on methodological grounds, but re-estimates employing more refined procedures have yielded the same results. See Peter D. McClelland, "The Cost to America of British Imperial Policy," *American Economic Review*, 59 (1969): 370–81; and Joseph D. Reid, Jr., "On Navigating the Navigation Acts with Peter B. McClelland: Comment," *American Economic Review*, 60 (1970): 949–53.

⁷ A detailed account of the data and estimating procedures is available from the author on request. The income loss is calculated by using tariff and transshipment cost data applied to the value of goods traded legally between 1797 and 1820. Indirect costs are estimated by assuming unit-price elasticity of demand. Like Thomas' estimates for British North America, the Mexican estimates here provide only a crude upper-bound measure of the burden of indirect trade.

⁸ According to Humboldt, the revenues of New Spain in 1803 amounted to some 20 million pesos, of which just over half were exported to Spain or to other Spanish colonies. Alexander v. Humboldt, *Political Essay on the Kingdom of New Spain*, trans. John Black, 4 vols. (London, 1811; reprint ed., 1966), 4: 224–29. Humboldt's figures do not coincide with those of the Royal Treasury Office in Mexico City. TePaske's reproduction of the accounts of the Real Caja in *La real hacienda de Nueva España* shows 1803 revenues at 39.1 million pesos, nearly double Humboldt's figure. Unfortunately, the Caja's accounts do not show which revenues were exported. Direct subsidies to other colonies, called *situados*, are shown at 2 million, close to Humboldt's estimate. Net revenues from royal monopolies were remitted to Spain by law; the accounts show gross revenues from this source at 5.8 million. It is likely that a large portion of the difference between Humboldt and TePaske lies in war loans not counted by Humboldt, the proceeds from which amounted to 10 million pesos in 1803. In the absence of precise export data from the original source, Humboldt's estimate of 10.1 million pesos for the fiscal burden is used in Table 2. Exports of bullion from New Spain declined drastically after the beginning of the independence wars in 1811. Thus, even if Humboldt's estimate for 1803 is too low, using it to approximate the annual average outflow for the entire period from 1797 to 1821 doubtless exaggerates the average fiscal burden over the whole period.

means negligible. But even if this estimate were five or ten times too low, independence would not have eliminated the gap in productivity between the Mexican and U.S. economies in 1800. Adding three pesos to Mexico's per capita income would still have left it at less than half that of the United States.

Many Mexicans in the nineteenth century, and not a few historians thereafter, have wondered why independence failed to stimulate the Mexican economy. Many have sought the answer to this question in the turmoil of the struggle for independence and the political instability that followed. Certainly these phenomena helped to depress the economy after 1810, but Mexico's income did not increase dramatically because independence had direct adverse economic results, which more than offset the benefits. Indeed, the continued depression in the crucial mining industry that persisted well into the nineteenth century was largely the result of an unanticipated cost of independence: the loss of assured supplies of mercury—indispensable for processing low-grade ores—that Spain had provided at low, fixed prices from the huge state-owned mercury mine at Almaden.⁹

Second, a major obstacle to economic advance cited in the conventional accounts is the system of land tenure or, more precisely, the organization of Mexican agricultural production into large estates called *haciendas*. This general explanation can be divided into a number of distinct assertions, of which at least two can now be rejected: (1) that the large estates of both the colonial period and the nineteenth century were inefficiently organized and badly managed and (2) that concentration of land ownership *per se* caused waste and misallocation of resources. Full discussion of the system of land tenure is impossible in so short a space. Fortunately, it is now possible to rely on a large body of *hacienda* studies, many of them completed within the past five years. Most of the estates studied were located in the Central Plateau from Oaxaca in the south to San Luis Potosí in the north. The records—including account books, correspondence, and property transfer documents—of more than fifty large *haciendas* have been intensively studied, while twice that number have been examined in less detail. These records often covered more than a century of activity. The colonial era (especially the first and last half centuries of Spanish rule) have fascinated historians most, but recent work has concentrated on nineteenth-century estates.¹⁰ In addition, a number of regional studies have been added to Charles Gibson's pathbreaking work on colonial Tlaxcala and the Valley of Mexico. These often contain the best information

⁹ Jean Paul Berthe, "El problema del abastecimiento del azogue a las minas de plata en México independiente," Paper presented to 41st International Congress of Americanists, Mexico City, 1974.

¹⁰ Juan Felipe Leal and Mario Huacuja produced an excellent bibliography in 1975 that covers most of the nineteenth-century work: "Fuentes para el estudio de la hacienda en México, 1856-1940," no. 10 in *Avances de Investigación*, Centro de Estudios Latinoamericanos, Universidad Nacional Autónoma de México (Mexico, n.d.). Enrique Semo covers the colonial period as well in his review of recent *hacienda* studies in "La hacienda mexicana y la transición del feudalismo al capitalismo," *Historia y Sociedad*, 2d ser., 5 (1975): 63-81. For seven recent studies, see Enrique Florescano, ed., *Haciendas, latifundios y plantaciones en América Latina* (Mexico, 1975). Also see, for an additional seven studies, Enrique Semo, ed., *Siete ensayos sobre la hacienda mexicana, 1780-1880*, no. 55 in *Colección científica: Historia*, Instituto Nacional de Antropología e Historia (Mexico, 1977).

available on nonestate agriculture as well as data on labor conditions and the status of tenants and sharecroppers.¹¹

Collectively, the *hacienda* and regional studies have transformed the traditional view of Mexican agriculture and estate management. Although primarily narrative descriptions of estate operations and organization, their impact has been as dramatic for Mexican history as the generation of econometric work on slave agriculture has been for the history of the southern United States. Not one estate owner has been found who might qualify as the sort of aristocratic, prestige-oriented, economic nincompoop once thought by many to be typical of Spanish American *hacendados*. Each was greedy in the ordinary way—even the managers of Church estates, for the income went for good works after all. Every one of them demonstrated a primordial desire to maximize income and to minimize production costs. During periods of prosperity, estate owners invested in their operations, experimented with new crops and new methods, and sought new markets. In periods of economic decline, they shifted from crops to livestock, reorganized their estates into tenancies, sold out to cut losses, or abandoned their holdings altogether. Given the relative costs of labor, capital, and especially management and supervision, their economic rationality was comparable to that of modern entrepreneurs. No evidence has yet been found to sustain the hypothesis that the estate sector of Mexico's agricultural economy wasted resources that might have been put to more productive use under different land-tenure conditions.¹²

The picture of colonial and nineteenth-century agriculture in Mexico that emerges from the evidence now available suggests two main conclusions. Estate agriculture enjoyed advantages not available to Indian villagers, small landowners, or tenant farmers: economies of scale, access to outside credit, information about new technologies and distant markets, a measure of protection from predatory officials, and greater security of tenure. But these advantages, important as they were, did not eliminate small-scale production because they were not sufficient to offset the high cost of recruiting and supervising labor. The large estates held a competitive advantage in the production of cattle, sheep, wool, food grains, pulque, sugar, and sisal. In other products that required very close supervision (or highly motivated workers) either to produce or to transport without great losses, the villages and small-scale producers held the advantage: fruits, garden products like tomatoes and chiles, silk, cochineal, small animals including pigs, poultry, eggs, and the like. Even cotton, tobacco, and wine were commonly produced

¹¹ See, for example, two of the most outstanding: William Taylor, *Landlord and Peasant in Colonial Oaxaca* (Stanford, 1972); and Claude Morin, "Croissance et disparités sociales dans une économie coloniale: Le Centre-ouest Mexicain au XVIII^e siècle" (Ph.D. dissertation, École Pratique des Hautes Études, University of Paris, 1974). And, of course, see Charles Gibson, *Texcala in the Sixteenth Century* (New Haven, 1952), and the masterful *Aztecs under Spanish Rule: A History of the Indians of the Valley of Mexico, 1519-1810* (Stanford, 1964).

¹² Only one of the *hacienda* studies directly addresses the question of productivity; and, without doubt, Ward Barrett's work provides an excellent model for historians concerned with the analytical issues posed here: *The Sugar Hacienda of the Marqueses del Valle* (Minneapolis, 1970).

by villagers and small landholders. Product specialization among units of varying size, location, and organization made Mexican agriculture more efficient than it would otherwise have been. The discipline of local and regional markets, moreover, acted to push the size of production units toward what a modern economist would describe as an "optimal mix." Far from distorting resource allocation in agrarian Mexico, concentration of landholding functioned to allow more efficient production of crops suitable for large units and did so without sacrificing the advantages of small-unit output for other produce.¹³

In the late nineteenth century the increasing availability of new transport and production technologies made the historic division of labor between estate and nonestate agriculture inefficient. The economic balance tipped dramatically in favor of the large *haciendas*. Agricultural productivity, stagnant during the half century after independence, increased rapidly as the estates expanded at the expense of the free villages and small landowners.¹⁴ Additional evidence may be adduced from comparative data. In 1800, between 70 and 80 percent of the Mexican labor force worked in agriculture to produce approximately 40 percent of the colony's gross product. Almost identical are the best estimates for the United States in the same year: slightly more than 80 percent of the labor force in agriculture produced approximately 40 percent of the nation's income. The gap in agricultural productivity between the two regions was exactly equal to the gap in nonagricultural productivity between the two economies. Mexico was only half as productive as the United States in both agricultural and nonagricultural production.¹⁵ In comparative terms, the agricultural sector of the Mexican economy did not, therefore, act as a drag on the nation's growth. Although this evidence is not decisive, it does at least suggest that historians should, in searching for obstacles that hindered Mexico's economy in the colonial era and the nineteenth century, look less at a single sector, important as it was, and more at conditions affecting the economy as a whole.

Third, the Roman Catholic Church purportedly retarded Mexico's economic growth during the colonial period and for at least three decades after independence, until the liberal revolution succeeded in expropriating the Church's wealth and reducing its role in the nation's political life. The anticlerical argument runs through volumes of highly charged prose and

¹³ On the production of small units, including villages, see William B. Taylor, "Haciendas coloniales en el valle de Oaxaca," *Historia Mexicana*, 23 (1973): 284-329; Celia Rabel, "San Luis de la Paz: Estudio de economía y demografía históricas (1645-1810)" (M.A. thesis, Escuela Nacional de Antropología, Universidad Nacional Autónoma de México, 1975); Brian R. Hamnett, *Politics and Trade in Southern Mexico, 1750-1821* (Cambridge, 1971); and Woodrow Borah, *Silk Raising in Colonial Mexico* (Berkeley and Los Angeles, 1943).

¹⁴ John H. Coatsworth, "Anotaciones sobre la producción de alimentos durante el porfiriato," *Historia Mexicana*, 26 (1976): 167-87.

¹⁵ For U.S. data, see Stanley Lebergott, *Manpower in American Economic Growth: The United States Record since 1800* (New York, 1964), 117; and Paul A. David, "The Growth of Real Product in the United States before 1840: New Evidence, Controlled Conjectures," *Journal of Economic History*, 27 (1967): 151-97. For Mexican estimates, see John H. Coatsworth, *From Backwardness to Underdevelopment: The Mexican Economy, 1800-1910* (forthcoming), chap. 2.

involves not only the Church's strictly economic activities but its political, social, and cultural influence as well. The portion of the argument that can now be rejected is that which assigns significance to the Church's strictly economic activities: (1) the tithe, (2) mortgage lending, and (3) ownership of real property.

The tithe was a 10 percent tax on gross output charged mainly on the agricultural and livestock production of the private estates. Like any direct tax today, the tithe reduced the profitability of agricultural enterprise. But by how much did this tax reduce Mexico's gross national product? It has often been implicitly assumed that agricultural production (and thus GNP) was reduced by the amount of the tax—that is, by 10 percent—as though the tithe collectors made off with a tenth of every harvest and burned it as a sacrifice. Even if this entirely inappropriate measure were adopted, the per capita drain of the tithe in 1800 would have amounted to less than half a peso—a little more than 1 percent of national income. By the time of independence (until 1833 when the tithe was abolished as a legal obligation of the citizenry), Church revenues from this source had already dropped to negligible sums.¹⁶

An appropriate measure of the impact of the tithe would have to take into account its effect on the profitability of private agricultural enterprise. By reducing profits in private agriculture, the tithe reduced employment and investment in that sector of the economy and pushed labor and capital into other, less productive activities. The negative effect on GNP is thus the difference between what the labor and capital pushed out of private agriculture actually produced elsewhere and what they would have produced (in the absence of the tithe) had they remained on the farms and estates. The amount of labor and capital pushed out of private agriculture was negligible; thus, the amount of this difference was close to zero, even when tithe collections reached a maximum at the end of the eighteenth century. Not only did the Church itself and the Indian villages produce a major portion of the colony's farm products and livestock (thus removing a sizable portion of agriculture from the full effect of the tithe), but differences in productivity between private agriculture and the rest of the economy indicate that nonagricultural pursuits were already more productive than agriculture. Therefore, the most important effect of the tithe was distributional: a larger portion of land remained in the hands of the Church and the Indian villages than would have been the case if the tithe had not been levied.¹⁷

Even when historians have realized that tithe collection did not directly reduce the gross national product, they have often suggested that the Church used this income unproductively: instead of investing its revenues in new industries and enterprise, the Church dissipated a tenth of the economy's

¹⁶ Michael Costeloe, *Church Wealth in Mexico: A Study of the "Juzgado de Capellanías" in the Archbishopric of Mexico, 1800–1850* (Cambridge, 1967), 14–18.

¹⁷ This discussion of the impact of the tithe is indebted to Pascual García Alba, "Los liberales y los bienes del clero," Seminar paper, El Colegio de México, 1974.

output on new religious construction or on the wages of priests. There are two problems with this contention. First, the multiplier effect of “unproductive” expenditure in modern times has proved reasonably effective in stimulating, rather than depressing, economic activity; and there is no reason to assume, *a priori*, that this effect was absent in colonial Mexico. Second, the Church did invest a sizable portion of its revenues in mortgage loans to private entrepreneurs. Indeed, the Church probably raised the rate of investment in the economy above what it would have been had the tithe revenues remained in private hands.

The Church’s role as the country’s chief banking institution has also been misunderstood. The Church earned a net income from the tithe, private donations, and its various properties. In addition, it acted as fiduciary agent for trust funds left in its care. The Church invested a large portion of its net income and all of the trust capital it managed, usually at 6 percent interest on the security of real property. Because it charged a low, nonmarket interest rate, the Church dominated the mortgage-lending market. What effect did this have on economic activity? Practically none at all. Once again, the main effect was distributional. The Church lost money when it lent funds below the market rate of interest, while the recipients of Church credit gained. It performed like a modern development bank, charging taxpayers to subsidize the accumulation of private capital. The Church imposed no legal or practical obstacles to prevent recipients from investing in factories rather than *haciendas* or high living.¹⁸ If factories were not built, reasons other than interference by the Church were involved.

Finally, the Church was a property owner. Studies of Church-owned estates suggest they were at least as well managed as those in private hands. The larger Church estates enjoyed considerable advantages, including a long-distance communication network which made it possible to plan both sales and purchases to take advantage of prevailing market conditions in widely scattered areas.¹⁹ Most Church estates after independence were rented to individuals, so the efficiency of these properties did not depend on Church management at all.²⁰ The major difference between the ecclesiastical and the private sectors lay in the Church’s exemption from most of the taxes levied on private enterprise by colonial and, later, national governments. There is no evidence, however, that public authority would have put revenues gained from taxing the Church to better use; and much evidence, in fact, suggests the contrary. After expropriation, for example, a large number of Church-supported charitable activities disappeared, so the short-run effect of moving Church properties into the taxable private sector seems to have been a drop in the welfare of poor people.

¹⁸ Costeloe, *Church Wealth in Mexico*, chap. 3.

¹⁹ Herman Konrad, “Santa Lucia, 1676–1767: A Jesuit Hacienda in Colonial Mexico” (Ph.D. dissertation, University of Chicago, 1973), 167–68.

²⁰ Jan Bazant, *Alienation of Church Wealth in Mexico: Social and Economic Aspects of the Liberal Revolution, 1856–1875* (Cambridge, 1971), 287.

THERE WERE TWO MAIN OBSTACLES TO ECONOMIC GROWTH in colonial Mexico which together explain most of the difference in productivity between the Mexican and U.S. economies in 1800: inadequate transport and inefficient economic organization—geography and “feudalism.”

Mexico's population and economic activity have always been concentrated in highland valleys and plateaus far from the sea. Inland waterways did not exist, nor could they have been constructed. The entire economy, therefore, depended on costly overland transportation to move goods and people. One example will suffice to show how geography affected transport costs and thus both the development of markets and the growth of productivity. In the late eighteenth century the Mining Deputation of Guanajuato estimated that this city received its supply of maize from estates located within a radius of ten leagues (fifty-five kilometers).²¹ At prevailing rates in this area, producers had to pay between one and one and one-third *reales* (0.125 to 0.166 pesos) per ton kilometer to ship goods commercially.²² The price of maize fluctuated widely, but for purposes of illustration it may be put at thirty pesos per ton.²³ Estates at the margin of this radius had to pay more than eight pesos (40 percent of the sale price of maize) just to carry it to Guanajuato. If maize producers had been able to ship by water to Guanajuato, at the same rates paid for shipments by canoe on the lakes surrounding Mexico City, Guanajuato's supply radius would have increased to between 485 and 725 kilometers.²⁴ The effects of such cheap transport can only be imagined, but the impact on national income would have been very large: increased regional specialization and division of labor, new centers of production previously undeveloped because of distance from centers of population and markets, greater reliance on markets to exchange products, greater mobility for both capital and labor, external economies due to better communication, and the like. The United States, of course, already possessed these advantages. Had Mexico shared them, the difference in productivity between the Mexican and U.S. economies would have been reduced, all other things being equal, by at least one-third.

This conclusion is based on what actually happened when railroads were constructed during the Porfiriato. Freight-transport costs fell to less than one-tenth of their prerrailroad levels. Social savings by 1910 amounted to at least 10.8 percent of gross domestic product, equivalent to one-third of the productivity gains of the Mexican economy between 1895 and 1910.²⁵ If Mexico had

²¹ For the supply radius for Guanajuato, see the citation in Eric Wolf, “The Mexican Bajío in the Eighteenth Century: An Analysis of Cultural Integration,” *Synoptic Studies of Mexican Culture*, Tulane University Middle American Research Institute, Publication 17 (New Orleans, 1955), 183.

²² Brading and Potash cite identical wagon freight rates when converted to ton kilometers. See David A. Brading, *Miners and Merchants in Bourbon Mexico, 1763–1810* (Cambridge, 1971), 16; and Robert Potash, *El Banco de Avío de México: El fomento de la industria, 1821–1846* (Mexico, 1959), 17.

²³ For the maize price cited, see both Brading, *Miners and Merchants in Bourbon Mexico*, 16, and Wolf, “The Mexican Bajío in the Eighteenth Century,” 17, where the figure is given as approximately eleven *reales* per *fanega*.

²⁴ For canoe rates on the Mexico City lakes, see Humboldt, *Political Essay on the Kingdom of New Spain*, 2: 174–75.

²⁵ For the railroad data, see John H. Coatsworth, *Crecimiento contra desarrollo: El impacto económico de los ferrocarriles en el porfiriato*, 2 vols. (Mexico, 1976).

been endowed with a system of rivers, like those along the eastern seaboard of the United States, a major portion of the U.S. advantage at the beginning of the nineteenth century would have been eliminated.

All other things were not, of course, equal. The viceregal government might well have decided to raise internal customs duties (*alcabalas*) to match reductions in transport costs. Or Madrid could have ordered the colonial government to deny licenses to entrepreneurs clamoring to take advantage of lowered transport costs and ready to invest in new productive enterprise. Or the crown could have decided to make transportation a royal monopoly (*estanco*, from which *estancar*, to stagnate) and control the supply of shipping and charge inflated rates. Or the Council of the Indies might have urged the king to protect the indigenous population by forbidding its employment in the production of goods sold in faraway places. Or, after multiple lawsuits lasting several decades, the Audiencia might have decided in favor of the petitions of muleteers, wagonmasters, and hotel keepers and ordered that all boats, rafts, and canoes in the colony had to be owned exclusively by former muleteers, wagonmasters, and hotel keepers who registered with the authorities and agreed to lend the king ten thousand pesos.

The very plausibility of measures such as these suggests a seriously deficient institutional environment for entrepreneurial activity. Indeed, the second of the two main obstacles to Mexican economic growth was inefficient economic organization. This term does not mean, for example, that private estates or mining companies wasted resources, given the environment they faced. It refers instead to an ensemble of policies, laws, and institutions that magnified, instead of reduced, the gap between the private and the social benefits of economic activity. During the colonial period and most of the nineteenth century, activities which could have contributed to economic growth were never undertaken because they promised too small a return to potential owners and producers. Either existing law and practice discouraged more productive enterprise or new laws and practices needed to protect and stimulate more productive activity never developed.²⁶

In the colonial period, legal constraints on the mobility of capital and labor inhibited the development of factor markets.²⁷ Minute public regulation of economic activity for fiscal and other purposes raised start-up costs and discouraged enterprise.²⁸ The judicial system increased the risks of entrepre-

²⁶ See Douglass North and Robert Paul Thomas, *The Rise of the Western World: A New Economic History* (Cambridge, 1973), chap. 1.

²⁷ For discussion of restrictions on geographic and occupational mobility, see Eric Wolf, "The Mexican Bajío in the Eighteenth Century," 179-83. Mobility of capital was impeded in numerous ways. Commercial activity, for example, was forbidden to certain occupational groups (e.g., clerics and public officials) and restricted for the majority of the population (Indians and individuals of mixed Indian and European ancestry). Guillermo Hernández Peñalosa, *El derecho en India y en su Metrópoli* (Bogotá, 1969), 408-12; and Jacinto Palleres, *El derecho mercantil mexicano* (Mexico, 1891), 167-91.

²⁸ See Wolf, "The Mexican Bajío in the Eighteenth Century," *passim*, for some examples. For an excellent survey of fiscal and economic policy in the eighteenth century, see Enrique Florescano and Isabel Gil Sanchez, "La época de las reformas borbónicas y el crecimiento económico, 1750-1808," in Alejandra Moreno Toscano et al., *Historia General de México*, 4 vols. (Mexico, 1976), 2: 183-301. Also see Enrique Semo, *Historia del capitalismo en México: Los orígenes, 1521-1763* (Mexico, 1973), 162-65.

neurial activity by failing to enforce a well-defined set of property rights.²⁹ Fiscal policy made transactions more costly, discouraged use of markets as a means for exchanging products, and contributed to the geographical isolation of those regional and local markets which did develop.³⁰ Royal monopolies on the production and distribution of many commodities distorted prices and reduced production.³¹ Investment by public authority or voluntary agencies in infrastructure or human capital was negligible. No general legislation existed to promote the realization of economies of scale through joint stock companies or corporations. Innovation was discouraged by a system of privileges that did not guarantee a return to inventors or investors in the application of new processes.³² Corporate exemptions from a portion of the risks and constraints imposed on the rest of society were distributed by the crown to favored groups and individuals, with the net effect of increasing the burden on others. The special courts established to hear cases involving members of each corporate group compounded the ordinary chaos of the judicial system with interminable litigation over which court was appropriate to hear each case and thus increased the uncertainty that plagued the legal environment.³³ The property of Indian villages, town councils, certain public bodies, entailed estates, and the trusts administered by the Church were defined by law as inalienable. The land thus held could not be used as collateral for mortgage loans, rented for extended periods of time to a single tenant, or sold to anyone.³⁴ However much evaded or even violated in practice, these constraints helped immobilize resources or divert them to less productive uses.

At the apex of this system of government sat the crown, whose power was constitutionally absolute. No rights of citizens and no law, regulation, or settled custom bound the king's freedom of action in the colonies. All legisla-

²⁹ In the eighteenth century, eleven different Spanish law codes, the earliest dating from 693 A.D., were still consulted to resolve disputes. Gustavus Schmidt, *The Civil Law of Spain and Mexico* (New Orleans, 1851), 152. In addition, mercantile law was partially codified in the Ordenanzas de Bilbao, updated at various points after their initial promulgation in 1459; and a new mining code, the Ordenanzas de Minería, was issued in 1783. John T. Vance, *A Guide to the Law and Legal Literature of Mexico* (Washington, 1945), 67, 203. Lesser codes existed and were applied to special areas of activity without ever clearly superseding more general legislation.

³⁰ Internal customs were established throughout the country to tax goods that passed between separate jurisdictions. Required permits (*guías*) for transporting foreign-made goods or goods destined for export had to indicate the precise route the merchandise would take and specify the amount of time to be consumed in transport. On arrival at the destination, a new document was issued that had to be physically transported to the origin of the shipment and submitted within a specified time to prove that the *guía* had been complied with. Deviation in time or route made shipments subject to confiscation. For most markets, permits and licenses were required or goods could be confiscated. Enrique Orozco, *La evolución de la legislación en la República* (Mexico, 1911), 7.

³¹ For a list of royal monopolies in the late eighteenth century, see Andres Lira, "Aspecto fiscal de la Nueva España en la segunda mitad del siglo XVIII," *Historia Mexicana*, 17 (1968): 388-89.

³² Juan de la Torre, *Legislación de patentes y marcas: Colección completa de todas las disposiciones que ha regido en México sobre esta materia, desde la dominación española hasta la época actual, concordada y explicadas* (Mexico, 1903), 1-5.

³³ One of the colony's most distinguished viceroys, Count Revillagigedo, put it most succinctly: "Everyone in his own court believes he will be better treated than in that of others, and thus all efforts are bent toward moving disputes and suits to one's own ground." As quoted in Jacinto Pallares, *El poder judicial: o Tratado completo de la organización, competencia y procedimientos de los tribunales de la República Mexicana* (Mexico, 1874), 35.

³⁴ Jose Maria Ots Capdequi, *España en América: El régimen de la tierra en la época colonial* (Mexico, 1959), chaps. 2-3.

tive and judicial acts derived their authority from the crown. The king could, and often did, grant individual exemptions from the application of his own laws or issue judicial decisions on appeal that controverted his own decrees in order to take into account the personal merits of the litigants. Not infrequently, the king's ministers, viceroys, and appellate courts acted in the same way.

The interventionist and pervasively arbitrary nature of the institutional environment forced every enterprise, urban or rural, to operate in a highly politicized manner, using kinship networks, political influence, and family prestige to gain privileged access to subsidized credit, to aid various stratagems for recruiting labor, to collect debts or enforce contracts, to evade taxes or circumvent the courts, and to defend or assert titles to land. Success or failure in the economic arena always depended on the relations of the producer with political authorities—local officials for arranging matters close at hand, the central government of the colony for sympathetic interpretations of the law and intervention at the local level when conditions required it. Small enterprise, excluded from the system of corporate privilege and political favors, was forced to operate in a permanent state of semiclandestinity, always at the margin of the law, at the mercy of petty officials, never secure from arbitrary acts and never protected against the rights of those more powerful.

This system of government made “free” enterprise impossible. It was not merely a matter of specific policies, laws, and institutions or their collective impact at a particular point in time that discouraged enterprise. The chief obstacle was the nature of the state itself, its operating principles, the basis for all of its acts. Mexico's economic organization could not have been made more efficient without a revolution in the relationship between the state and economic activity.

Unfortunately, the cost-benefit approach so helpful in identifying the impact of economic organization in theoretical terms cannot be applied quantitatively. It is possible to know the direction (positive or negative) of the effects of specific laws, policies, or institutions but not the magnitude of their impact. Direct measurement is impossible in practice, even to distinguish the relative importance of the various components of the system. Some advances may come from the aggregation of individual case studies at the enterprise level, but these may not be of much help in estimating the loss from economic activities that never got started in the first place. Treating the impact of economic organization as a residual is scarcely satisfactory, especially when the size of the residual is suggested by an international comparison rather than an estimate of Mexico's own potential for growth. Nonetheless, a starting point for future work might well be the hypothesis that most of the gap between the Mexican and U.S. economies in 1800 was due to differences in economic organization.

THE TWO MAIN OBSTACLES TO ECONOMIC GROWTH in colonial Mexico—inadequate transport and inefficient economic organization—could have been

eliminated early in the nineteenth century. Independence in 1821 emancipated the country from the source of the policies, laws, and institutions that inhibited enterprise. And railroad technology developed in the 1830s and could easily have been imported by 1840.³⁵ By the time of independence, the liberal Spanish Cortes, established to resist the Napoleonic invasion and later revived under popular pressure, had already eliminated many important constraints on economic activity. Ethnic distinctions between citizens in employment, taxation, and justice were abolished; corporate property rights were restricted to the Church and the Indian villages and town councils; the number of royal monopolies was reduced and their activities were curtailed; the corporate privileges of certain groups, including most of the guilds, were eliminated; efforts were made to streamline the judicial system; and revision of the antiquated law codes was begun.³⁶ But then Mexico plunged into a half century of political, social, and international warfare. The collapse of stable government nullified the potentially positive effects of the changes that accompanied independence and deprived both the new government and the private sector of the resources needed to improve transportation.

Mexican independence came through a virtual coup d'état by the colony's Creole elite, carried out largely to separate Mexico from the liberalizing process under way in the mother country. For the next half century, repeated efforts were made to recreate the arbitrary centralism of the colonial state. The principal proponent of these conservative efforts was a limited social group of major landowners and industrialists in the center of the country (often residents of Mexico City), who had been the principal beneficiaries in the colony of the crown's interventionism or who, like the large merchant houses of the capital, sought to regain privileges the crown itself had abolished in the reforms of the late Bourbon era. Allied with this group were the Church, seeking to preserve its privileged status and to reverse the anti-clericalism of the later Bourbon kings, and the new professional army, endowed with privileges in the early constitutions, whose chief *raison d'être* was to be found as enforcer of the new centralism against regional demands for greater autonomy and liberal clamorings for reform.³⁷

Traditional accounts of Mexico after independence have attributed the conservative orientation of the new country's early governments to the influence of a "feudal" or "semifeudal" landowning class. Early national governments were weak. The nation disintegrated into a multiplicity of regional satrapies controlled by local *caudillos*. The parallel to Europe's Middle Ages seemed very apt. The chief problem with this interpretation has been and remains a lack of systematic data on the social composition of the political

³⁵ The first concession issued by the Mexican government for construction of a railroad was dated 1837. The line that was envisioned, from Mexico City to Veracruz, was not completed until 1873. John G. Chapman, *La construcción del Ferrocarril Mexicano, 1837-1880* (Mexico, 1974), 22-25, 160.

³⁶ For the principal acts of the Cortes and the major legislative changes enacted in Mexico after independence, see Manuel Dublan and José María Lozano, *Legislación mexicana o colección completa de las disposiciones legislativas expedidas desde la independencia de la República*, 1 (Mexico, 1876).

³⁷ Michael Costeloe, *La Primera República Federal de México, 1824-1835* (Mexico, 1975), 438-39; and Jan Bazant, *A Concise History of Mexico from Itz'atzo to Cárdenas, 1805-1940* (Cambridge, 1977), 74-77.

forces that fought for control over the new nation's destiny. Aside from the small group of magnates in the capital, there is little evidence to suggest widespread support for conservative centralism among Mexico's landowners. Many—including important regional chieftains—supported the liberal cause, because it promised less interference by the national government in local affairs. This support, in turn, aided the Church in its efforts to convince the Indian population to support the capital against the local liberals.

Feudalism, in the broad conception of Marxist historians, is usually linked to serfdom. Serfs were peasants permanently attached to the persons or properties of aristocrats. They labored with their own implements and were obligated to yield up a portion of their product or to contribute a portion of their labor to the lord. In Mexico, however, legal serfdom did not exist and recent research has demonstrated that debt peonage (often assumed to be its New World equivalent) was effectively practiced only in parts of the sparsely populated geographical extremities of the country. Indeed, Mexico's landowners enjoyed none of the privileges of the old European nobility, either before independence or after. Spanish *raison d'état*—principally the fear of an American nobility rising to claim sovereignty over New World populations—prevented any such development. Indian access to land came from the crown in the form of corporate, and thus inalienable, land grants to villages. The crown never countenanced legal obligations between Indian villagers and landowners, save those regulated by royal decree and administered by royal officials. The royal courts and the tenacious resistance of the villagers themselves prevented the incorporation of villagers and their lands into the *haciendas*. Preservation of nonexistent servile obligations or other privileges played no role in determining the social composition of the warring parties in independent Mexico.³⁸

If Mexico's landowners were not privileged, neither were they powerless. By designating the villages as corporate entities, the crown had virtually tied the bulk of the Indian population to specific pieces of real estate. Geography, culture, and lack of communication restricted Indian mobility still further. Most Indian villagers worked as seasonal labor on the great estates in the vicinity of their homes. Often, they did so out of need—village land grants in the sixteenth and seventeenth centuries were inadequate for the rising population in later periods. In the eighteenth century the royal courts became less protective of village land titles as the large estates expanded to profit from the colony's prosperity. In many areas seasonal labor on the estates acquired the force of habit over many years or continued as a form of rent charged for access to estate water, salt, or woodlands.

Coercion, however, was widespread and pervasive. Usually it involved various kinds of pressures applied by landowners in cooperation with local civil and ecclesiastical authorities. The stratagems employed varied from place to place, as did the success of the landowners who employed them. The

³⁸ On peonage in the far north and in the south of the country, see Friedrich Katz, "Labor Conditions on Haciendas in Porfirian Mexico: Some Trends and Tendencies," *HAHR*, 54 (1974): *passim*. For an earlier period, see the *hacienda* studies reviewed in the literature cited in note 10.

colonial authorities resisted these informal attempts to obligate local villagers but their efforts were never entirely effective, for the landowner's capacity to manipulate the local environment was always decisive. Far from desiring a restoration of Bourbon centralism, most landowners wanted to be allowed a free hand to control their immediate surroundings.

The social composition of what became the liberal movement has not received adequate attention from researchers. The only significant study of this question—a survey of the delegates to the Constitutional Convention of 1856–57—has revealed that most of the participants were either lawyers or generals in the liberal forces.³⁹ In the port cities on the Gulf of Mexico, support for liberal governments was apparently quite strong, which suggests ties to merchants and tradesmen in addition to local landowners and *caudillos*. The liberals also appealed to the “professional classes, lawyers, doctors, small property owners, merchants, the middle ranks of the clergy and military. . . .”⁴⁰ While the thrust of liberal demands for change—at least those emanating from liberal political leaders who assumed command of the movement at the national level—was for institutional change modeled on the example of the United States and Western Europe, Mexico's bourgeoisie—under any definition of the term—constituted a small, weak, and highly fragmented social grouping. Neither landowners nor capitalists can be said to have formed a national governing class in independent Mexico. With more than fifty changes of government in a half century, no group effectively dominated national government.⁴¹

Because it fought to prevent institutional change, the Church did constitute an important obstacle to economic growth, even though its strictly economic activities did not directly impede progress. The lack of evidence demonstrating systematic support for the conservative cause by Mexico's landowners, however, makes it impossible—even in this general sense—to describe the system of land tenure as a significant obstacle to economic advancement. The social base of Mexico's conservative movement was not determined by the nature of rural social relations but by the pattern of relations between a narrow stratum of the economic elite—however their fortunes were made—and the central government. In both colonial and independent Mexico, Enrique Semo's observation was brilliantly precise. “Feudalism in Mexico,” he wrote, “was strongest at the level of superstructure.”⁴²

³⁹ Richard N. Sinkin, “The Mexican Constitutional Congress, 1856–57: A Statistical Analysis,” *HAHR*, 53 (1973): 1–26.

⁴⁰ Costeloe, *La Primera República Federal de México*, 439.

⁴¹ The most disastrous consequence of Mexico's relative economic backwardness and prolonged internal strife was the loss of one-half of the national territory to the United States. The magnitude of the loss became apparent almost immediately. The California “Gold Rush” began in 1849, a year after Mexico ceded that territory in the Treaty of Guadalupe Hidalgo. By the beginning of the century, the mineral output alone of the lost territories exceeded the GNP of the Mexican Republic. Mexico did not have the capacity to develop its vast territories, assimilate immigrants, and repel aggression in the mid-nineteenth century. Nor did any of Spain's former colonies at that early date. Had Mexico encountered a less powerful and warlike neighbor than the United States, the vast wealth of the northern half of the country could have altered the whole course of the country's economic history later on.

⁴² Enrique Semo, “El desarrollo del capitalismo en la minería y la agricultura de la Nueva España,” *Historia y Sociedad*, 5 (1969): 5.

FOR A MULTITUDE OF REASONS that need not detain us here, liberalism emerged triumphant from Mexico's postindependence turmoil. The last hope for restoration of the colonial pattern of government ended when the short-lived regime of Emperor Maximilian (1862-67) embraced an essentially liberal program and moved rapidly to remove the same obstacles to capitalist development that his liberal enemies hoped to abolish. Not only did Maximilian promulgate the nation's first modern commercial code to replace the Ordenanzas of Bilbao, but his government used French aid to push construction of the nation's first railroad as well.⁴³

When the apparatus of national government fell to the liberal regime of Benito Juárez in 1867, the first and most important step in the transformation of property rights proclaimed in the Constitution of 1857 was already an accomplished fact. Most of the wealth of the Church was now in private hands. Little more was accomplished during the decade of the Restored Republic. The liberals did manage to produce a new civil code (1870) to recognize the new status of Church-state relations, but the commercial code was revoked and nothing was enacted to replace it. Tariff reform, which affected the largest source of federal government revenues, was enacted by Congress in 1872, but modernization of the tax system and reform of the antiquated and inefficient treasury were postponed. The judiciary was reorganized and purged, but the main qualification for appointment was loyalty to the new regime and the judicial system remained as chaotic as ever. The Juárez and Lerdo regimes lacked the resources to repair roads, subsidize railroad construction, build schools, or lower taxes. The Veracruz-Mexico City rail line was finally completed in 1873, but the debt-ridden company that owned it charged rates only slightly below the cost of shipments by wagon or mule.⁴⁴

When Porfirio Díaz seized power in 1877, nothing had been done to reform the colonial mining code since the 1820s, when Congress abolished the mining guild and relaxed the prohibition on foreign investment. No legislation existed to encourage the formation of corporations with limited liability. No banking laws were passed, except for concessions to particular banks. No mortgage-credit law existed to protect long-term investment and replace the spiritual sanctions on which the Church had relied. A modern patent law did not exist. Despite constitutional provisions that specifically outlawed them, colonial fiscal measures like the internal customs still provided most of the revenue for state and municipal governments. Economic activity of all kinds still required special permits and licenses for which special taxes and fees were charged. Though Church wealth had been expropriated, the corporate holdings of the Indian villages remained unaffected throughout most of the country. The liberal movement had destroyed the political power of the Church, seized the

⁴³ On railroads during the "second" empire, see Chapman, *La construcción del Ferrocarril Mexicano*, chaps. 5-7. On policy and law under Maximilian, see José Luis Martínez, "México en busca de su expresión," in Josefina Zoraida Vasquez et al., *Historia General de México*, 3: 138-62.

⁴⁴ Francisco Calderón, *La República Restaurada: La vida económica*. in Daniel Cosío Villegas, ed., *Historia moderna de México* (Mexico, 1965), contains the most exhaustive account of economic policy and legislation during this period.

apparatus of government, and changed the constitution. But a new superstructure of laws and institutions for a capitalist society had yet to emerge.

The Porfirian military coup occurred at a fortuitous moment. In a short time, the Díaz regime issued major railway concessions for lines running across the Central Plateau and northward to the U.S. border. Railroad concessions raised land values along projected routes and precipitated widespread usurpations of Indian village lands by estate owners and land companies. Needless to say, the railroad companies reported no difficulties in recruiting thousands of propertyless wage laborers for the massive construction projects that got under way in late 1880. In the next three years, nearly five thousand kilometers of track were built by tens of thousands of Indian workers, many of whom had only recently been driven from their lands. As cheap transport and the nation's new prosperity revived the profitability of the estates, expropriation of village lands progressed apace. The dispossessed villagers swelled the ranks of the nation's landless rural and urban proletariat.⁴⁵ Capitalist modernization had begun.

As railroad lines spread throughout the countryside and the first signs of massive foreign interest in Mexican resources appeared, a series of major legislative reforms was enacted. In 1884, Congress passed a new commercial code, the single most important piece of economic legislation since independence. (The new code had to be reformed—ironically, the revisions were modeled after Spain's 1885 code—in 1889, largely because it did not make adequate provision for limited-liability corporations.) In 1887, a new mining code followed. Banking, first included in the commercial codes, became the object of special legislation in 1897 and 1908. Reform of the fiscal system, begun in 1881 with reorganization of the treasury, continued in stages for the rest of the decade until new tax laws, tariff schedules, and public-debt reorganization had been achieved. After more than a decade of virtual isolation, the Mexican government signed commercial treaties, first with the United States and, later, after successful renegotiation of the foreign debt, with all of the European powers.⁴⁶

THE SIMULTANEOUS DEVELOPMENT OF TRANSPORTATION and a more efficient economic organization made possible the economic growth of the Porfirian era. That growth had characteristics that made Mexico's advances in this period markedly different from the economic and institutional development of the industrial economies of the North Atlantic. While Mexico was resolving its internal conflicts, exacerbated as they were by foreign wars and invasions, the Industrial Revolution brought Europe and the United States to new heights of productivity. The main significance of Mexico's greater relative

⁴⁵ John H. Coatsworth, "Railroads, Agrarian Protest, and the Concentration of Landholding in the Early Porfiriato," *HAHR*, 54 (1974): 48–71.

⁴⁶ Legislative and administrative changes during the Porfiriato are surveyed in appropriate chapters of Daniel Cosío Villegas, ed., *Historia moderna de México: El Porfiriato, La vida económica*, 2 vols. (Mexico, 1965), 1: chap. 3, 2: chaps. 7–9.

backwardness lay in the vast comparative advantages of foreign technology and resources for the development of Mexico's own economy. No Mexican government, whatever the social composition of its leaders and supporters, could have long resisted the benefits offered by foreign participation in Mexico's economy. What Mexico might have done on its own when it first won independence could no longer be done without a far greater sacrifice of palpable immediate gains by the 1870s.

The participation of foreign capital in Mexico's first period of sustained capitalist growth had five main consequences. First, the construction of railroads and subsequent development of major export industries interrupted the gradual disintegration of the large estates into smaller units, which had begun in 1810 with the outbreak of the independence movement. The new prosperity also ensured that the properties expropriated from the Church passed into the hands of large-scale operators. Second, the new industries developed by foreign capital and by Mexican entrepreneurs with foreign financing also operated on a large scale. In both agriculture and industry, large-scale units ensured more rapid growth. They also defined the nature of the institutional changes Mexico adopted to facilitate capitalist development.

Third, Mexico's numerous class of petty tradesmen and small-scale producers were therefore assigned a position in Mexico's new society similar to that which it had occupied in the colonial era. Since it was clearly marginal to the country's economic progress, it suffered from the same arbitrary treatment and operated in the same netherworld of semiclandestinity as before. Nor did that numerous class of independent farmers envisioned by some liberal leaders ever emerge. Without either force, Mexican governments reverted to authoritarian models from the colonial past, despite the new institutions of the liberal era, and spent their increasing resources on more important projects than the development of the nation's human resources.

Fourth, the Revolution of 1910 was produced by the discontent of precisely those elements in Mexican society whose importance in the nation's political and social life would have increased, rather than diminished, in the absence of foreign resources: the dispossessed villagers who never became family farmers and the "middle sectors" that continued to suffer exclusion from both political and economic opportunity. Fifth, foreign resources established a pattern of economic activity that fixed Mexico's position in the world economy at the most unfavorable moment in the nation's economic history. Foreign resources attracted their Mexican counterparts to produce, labor, and invest in activities made profitable by Mexico's short-term comparative advantage in the production of raw materials and agricultural products for export. It may be doubted, of course, that Mexico's present condition would be more favorable had the nation followed a different course. But it is clear enough, in any case, that the path Mexico did follow promised no more than a long-term dependence on foreign technology, resources, and markets.

Pancho Villa and the Attack on Columbus, New Mexico

FRIEDRICH KATZ

ON MARCH 9, 1916, A MEXICAN RAIDING FORCE of five hundred men attacked the town of Columbus, New Mexico, to cries of “Viva Villa” and “Viva México.” According to all available evidence, the leader of the attack was the Mexican revolutionary general Francisco “Pancho” Villa. The raiders were repulsed by units of the 13th U.S. Cavalry, garrisoned in Columbus, after a six-hour battle. More than one hundred Mexicans and seventeen Americans died in the fighting. The United States response to the attack came quickly. Within one week a punitive expedition, initially composed of four thousand eight hundred men (later increased to ten thousand men) commanded by General John J. Pershing, invaded the Mexican state of Chihuahua under orders from President Woodrow Wilson to capture the leader and instigator of the Columbus raid, Pancho Villa. The Pershing expedition proved to be both a political and a military disaster. In political terms, it brought the United States to the brink of war with Mexico and antagonized large segments of the Mexican public. In military terms, it failed completely in its attempt to capture Villa. On February 5, 1917, the punitive expedition withdrew into the United States, having failed even to catch sight of its elusive prey.¹

U.S. military intervention in Latin America has been all too common; the Villa raid on Columbus is the one instance of Latin American military intervention in the United States. Perhaps for that reason, it has been the subject of widespread speculation and controversy. What led Villa to under-

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¹ There is a very large body of literature on the punitive expedition into Mexico. For some of the main works written by Americans, see Haldeen Braddy, *Pershing's Expedition in Mexico* (El Paso, 1966); Clarence Clendenen, *The United States and Pancho Villa* (Port Washington, N.Y., 1971); Arthur S. Link, *Wilson: Confusion and Crisis, 1915-1916* (Princeton, 1964), and *Wilson: Campaigns for Progressivism and Peace* (Princeton, 1965); Herbert Molloy Mason, Jr., *The Great Pursuit* (New York, 1970); Donald Smythe, *Guerrilla Warrior* (New York, 1963); Michael L. Tate, “Pershing's Punitive Expedition: Pursuer of Bandits or Presidential Panacea?” *The Americas*, 32 (1975): 46-72; and Frank Tompkins, *Chasing Villa* (Harrisburg, Pa., 1939). For two Mexican works, one a monograph and the other a collection of documents by authors sympathetic to Carranza, see Alberto Salinas Carranza, *La Expedición Punitiva* (Mexico, 1936); and Josefina E. de Fabela, ed., *Documentos históricos de la Revolución Mexicana XII Expedición Punitiva*, 2 vols. (Mexico, 1967-68). For works by Mexican authors sympathetic to Villa, see Alberto Calzadiaz Barrera, *Porque Villa atacó a Columbus* (Mexico, 1972); Nellie Campobello, *Apuntes sobre la vida militar de Francisco Villa* (Mexico, 1940); and Federico Cervantes, *Francisco Villa y la Revolución* (Mexico, 1960).

take this seemingly quixotic adventure? The dispute among historians as to his motives shows no signs of abating. Among the reasons usually given, the most prominent are (1) Villa's desire to revenge himself on the Wilson administration for its recognition and support of his enemy, Mexican President Venustiano Carranza; (2) Villa's desire to revenge himself on U.S. arms speculators who had cheated him; (3) Villa's wish to capture supplies of food and arms; and (4) Villa's hope of obtaining German arms and support in return for his attack against the United States.² Most historians have viewed the attack on Columbus as an act of irresponsibility at best and of complete irrationality at worst. Some have suspected Villa of harboring an almost pathological hatred of the United States after the Wilson administration had repaid him for his initial support of U.S. aims and his refusal to interfere with U.S. business interests by aiding his rivals. In any case, the great disparity between what Villa might have expected to gain and what losses Mexico could expect to suffer as a result of his attack on the United States has led many to interpret the raid as little more than the revenge of a reckless desperado.

New documentary evidence suggests that Villa was neither as irrational nor as irresponsible as is commonly suggested. The reasons and circumstances usually adduced to explain his decision to attack Columbus were at best secondary in importance. The primary motivation was Villa's firm belief that Woodrow Wilson had concluded an agreement with Carranza that would virtually convert Mexico into a U.S. protectorate. Although such an agreement never existed, Villa had reasonable grounds for supposing that it did. In light of this supposition, his actions can no longer be construed as the blind revenge of an unprincipled bandit. They must be viewed as a calculated effort to safeguard what Villa believed others had blindly surrendered—Mexico's independence.

TO UNDERSTAND HOW VILLA CAME TO BELIEVE in the existence of an agreement for which he never received any direct evidence, one must delve somewhat into the history of the constitutionalist movement, of which he was a part, and of its relations with the U.S. government and U.S. business interests. The constitutionalist movement emerged in northern Mexico in March 1913 after the overthrow and assassination of revolutionary President Francisco I. Madero by conservative forces led by General Victoriano Huerta. The immediate

² For interpretations of the motives for Villa's attack on Columbus, see, in addition to the works cited in the previous note, Charles Harris III and Louis R. Sadler, "Pancho Villa and the Columbus Raid: The Missing Documents," *New Mexico Historical Review*, 50 (1975): 335-47; Larry A. Harris, *Pancho Villa and the Columbus Raid* (El Paso, 1949); Friedrich Katz, "Alemania y Francisco Villa," *Historia Mexicana*, 12 (1962): 83-103, and *Deutschland, Diaz, und die mexikanische Revolution* (Berlin, 1964); Francis J. Munch, "Villa's Columbus Raid: Practical Politics or German Design?" *New Mexico Historical Review*, 44 (1969): 189-214; James A. Sandos, "German Involvement in Northern Mexico, 1915-1916: A New Look at the Columbus Raid," *Hispanic American Historical Review*, 50 (1970): 70-89; Barbara Tuchman, *The Zimmermann Telegram* (New York, 1958); and E. Bruce White, "The Muddled Waters of Columbus, New Mexico," *The Americas*, 32 (1975): 72-92.

problem was to find ways to finance its war against the Huerta government. The short-lived Madero movement of 1910–11 had been financed essentially by funds “borrowed” by Madero’s brother, Gustavo, from a French railroad company, by forced loans levied on wealthy Mexicans in the northern parts of the country, and perhaps even by contributions made by U.S. oil and other business interests.³ From the beginning, it was clear to the various leaders of the constitutionalist movement that raising funds on such a makeshift basis would not suffice for them. They faced a longer and more exhausting struggle against a more vigorous and determined enemy than that opposed by the Madero movement.

Two very different strategies were evolved by the revolutionary leaders in northern Mexico to meet the expenses of the fight. Pancho Villa, whose sway extended essentially over Chihuahua and Durango, sought to shift the burden of financing the revolution onto the old Porfirian upper classes and some weaker groups of foreigners—above all, the Spaniards. At first he contented himself with exacting forced loans. By the end of 1913 he went further, confiscating all the landholdings of the Chihuahuan upper classes. These were managed on the state’s behalf by state administrators. Many Spaniards were expelled from the Villa-controlled territories and much of their land was confiscated.⁴ The property of other foreigners, especially Americans, was left untouched. Initially, Villa burdened them with neither heavy taxes nor contributions. Carranza’s financial strategy was very different. He strongly opposed confiscating the wealth of Mexico’s upper class, even though he was forced on occasion to accede to confiscations carried out independently by some of his generals. When Carranza could not prevent the confiscation of estates, he did everything in his power to emphasize the temporary character of such measures and prohibited the land from being divided among the peasantry. As soon as he could, he returned these properties to their owners.⁵ In order to meet his financial problems, Carranza decided to shift as much of the burden of new taxes as possible onto foreign companies. Since U.S. investments

³ See, especially, Stanley R. Ross, *Francisco Madero, Apostle of Democracy* (New York, 1955). For possible links between Madero and the oil companies, see Kenneth J. Grieb, “Standard Oil and the Financing of the Mexican Revolution,” *California Historical Society Quarterly*, 45 (1971): 59–71; and Katz, *Deutschland, Diaz, und die mexicanische Revolution*, 186.

⁴ Cervantes, *Francisco Villa*, 79; Friedrich Katz, “Agrarian Changes in Northern Mexico in the Period of Villista Rule,” in *Contemporary Mexico* (Papers of the Fourth International Congress of Mexican History, October 1973), ed. James W. Wilkie, Michael C. Meyer, and Edna Monzón de Wilkie (Mexico, 1976); and John Reed, *Insurgent Mexico* (1914; reprint ed., New York, 1969), 122.

⁵ The problem of the confiscation of *haciendas* by revolutionary authorities and the later return of these estates to their former owners by the Carranza government is one of the most important and least studied aspects of the Mexican Revolution. See Katz, “Agrarian Changes in Northern Mexico,” and Douglas Richmond, “The First Chief and Revolutionary Mexico: The Presidency of Venustiano Carranza, 1915–1920” (Ph. D. dissertation, University of Washington, 1976), 60–61. Carranza did not very frequently deal with this problem in public, preferring instead to stress his commitment to agrarian reform; on one of the few times he did publicly address this issue, he spoke to the Constitutional Convention at Queretaro in 1917. See *Informe del C. Venustiano Carranza, Primer Jefe del Ejército Constitucionalista, Encargado del Poder Ejecutivo de la República: Leído ante el Congreso de la Unión en la Sesión de 15 de Abril de 1917* (Mexico, 1917). He very definitely showed his opposition when he reprimanded General Lucio Blanco, who in 1913 divided the lands of the Hacienda de los Borregos among the peasants. See Armando de María y Campos, *La Vida del General Lucio Blanco* (Mexico, 1963), 68.

predominated in northern Mexico, this produced repeated conflicts with U.S. business interests and, ultimately, with the U.S. government.

Villa solved his financial problems by dealing less reverently with Mexican-owned private property than Carranza, a policy that reflected, in part, the difference in the social origins of the two men. Villa grew up as a sharecropper and later became a cattle rustler; Carranza—at the other end of the social spectrum—was born and raised a *hacendado*. Villa's irreverence toward private property also reflected, however, the character of the region he controlled, in contrast to the states of Coahuila and Sonora dominated by Carranza. The latter's domain possessed a relatively liberal group of *hacendados*, many of whom had actively supported the Madero movement. Almost all of the Madero leaders in Coahuila and Sonora (for example, the Madero family, Carranza) were recruited from that group. By contrast, Villa's region had a far less liberal class of *hacendados*. Most of them fiercely opposed the Madero movement and showed themselves signally unprepared for even the most moderate reforms. No prominent *hacendados* supported the Madero movement in Chihuahua; in fact, they were the first to take an active stand against Madero by aiding the Orozco rebellion of 1912.

No revolutionary movement could have survived in Chihuahua without destroying both the economic and political power of the traditional ruling oligarchy. Realizing this, Villa expropriated Chihuahua's large estates, promising the peasants that the land would be divided up as soon as the victory of the Revolution had been ensured. The confiscations had the additional tactical advantage of diverting much of the popular support Orozco had been able to secure through vague promises of land reform.⁶ Thanks to his expropriation of large cattle estates in Chihuahua and, later, of the cotton grown by Spanish landowners in the Laguna area, Villa could count on sufficient resources to finance his revolution for most of 1914 without having to put pressure on U.S. companies, a fact which helps to explain why taxes on those companies were far higher in Carranza's zone than in Villa's.⁷ Through his restraint toward U.S. business, Villa doubtless hoped to gain access to U.S. arms and, eventually, even to gain official recognition by the U.S. government. Yet he did not cater to the interests of U.S. businessmen. Elsewhere in Mexico, U.S. companies purchased land and other assets from members of the upper class who panicked as the Revolution progressed and were ready to sell out at cut-rate prices. Villa's expropriations prevented this process from occurring in Chihuahua. Despite the obstacle to further Americanization, Villa's impressive ability to control his troops and his restrained taxation of U.S. companies won him a good measure of initial U.S. support.⁸

⁶ Pascual Orozco, a former leader of the Revolution of 1910–11, staged an uprising against Madero in 1912. In his program he emphasized the need for agrarian reform, though he never distributed any land. The Orozco revolt frequently has been linked to the large estate owners in Chihuahua. See Michael Meyer, *Mexican Rebel: Pascual Orozco and the Mexican Revolution* (Lincoln, Neb., 1967).

⁷ Harvey O'Connor, *The Guggenheims: The Making of an American Dynasty* (New York, 1937), 336–37. In a manifesto issued to the Mexican people after the United States recognized Carranza, Villa insisted that he had treated U.S. business interests far better than had his rival. See *Vida Nueva* (Chihuahua), Nov. 21, 1915.

⁸ One of America's most famous muckrakers, Lincoln Steffens, wrote in his autobiography, "The Reds in New York who were watching Mexico were on Villa's side, but the only reason they gave was that he was

How did Villa's tolerance toward U.S. business interests affect the policies of the Wilson administration? Unlike his relations with the business interests, Villa's relations with the administration have been the subject of intensive research.⁹ Wilson was willing to throw U.S. support behind any serious contender for power in Mexico who could meet five criteria: (1) a demonstrable inclination to carry out social and political reforms to stabilize the country, including some kind of agrarian reform (Wilson never specified how far-reaching such a reform had to be and at whose expense it was to be carried out); (2) respect for parliamentary institutions and an intention to carry out free elections as Madero had done; (3) dedication to the system of free enterprise without being subservient to any single business interest; (4) strict respect for U.S. property rights and no partiality toward European and, above all, British interests; and (5) a commanding personality, strong enough to

at least a bandit, a Barabbas, whereas Carranza was a respectable, landowning bourgeois. Jack Reed talked that way, and he later went in on Villa's side. I thought of a trick I used to practice in making a quick decision in politics at home. I'd ask Wall Street, which is so steadily wrong on all social questions. If I could find out which side Wall Street was on, I could go to the other with the certainty of being right. So I inquired down there for the big business men with Mexican interests, called on and invited several of them to luncheon. They came eager to 'start me off right.' And they agreed that Villa was the man. Their reason? 'Well, you see, we have tried out both of them and Carranza, the ———, we can't do a thing with him. He won't listen to reason. Obstinate, narrow-minded, proud as hell, he has thrown us out again and again. Whereas Villa . . . You mustn't get the idea that just because he's a bandit he's no good. We have had him seen and—he's all right, Villa is.' " Lincoln Steffens, *The Autobiography of Lincoln Steffens* (New York, 1931), 715. Steffen's statements are only partly true. Yet there is little doubt that some of the most important U.S. mining companies with large interests in Mexico supported Villa. This was certainly true of the largest of them—The American Smelting and Refining Co. See O'Connor, *The Guggenheims*, 334–36. J. D. Ricketts, whose company controlled the huge Cananea mine in Sonora, had a similar attitude. "Of course, the best man that they found is Villa," he wrote to General Hugh Scott in January 1915. Ricketts to Scott, Jan. 20, 1915, Library of Congress, Scott Papers, box 15 (general correspondence). For a brief period, William Randolph Hearst, who owned a huge ranch in Chihuahua, also favored Villa. In an editorial published on September 26, 1914, entitled "Pancho Villa, the Strong Man of Mexico," Hearst's New York *American* blamed Wilson for not supporting Villa as president of Mexico. This policy did not last long, and on June 3, 1915, before U.S. recognition of Carranza, the *American* referred to Villa as a bandit and called for U.S. intervention in Mexico. Hearst's change of attitude was probably due to the imposition of higher taxes on foreign holdings by Villa. Not all U.S. business interests favored Villa. The oil companies, at least through 1914, were far more sympathetic to Carranza. Edward Doheny, president of Mexican Petroleum, which had close links with Standard Oil, stated before a Senate hearing in 1919 that his company, like many others doing business in Mexico, had supported Carranza from 1913 onward and paid him \$685,000 as an advance on future taxes. See United States Senate Documents, Foreign Relations Committee, Investigation of Mexican Affairs, Reports and Hearings, 1 (66th Congress, 2d Session, Senate Document no. 285; Washington, D.C., 1920): 278. Henry Clay Pierce, head of another oil company, also linked to Standard Oil, established close relations with Carranza. Both he and the Mexican revolutionary chief retained the same lawyer, Sherbourne G. Hopkins, who—according to Paul von Hintze, the German Ambassador in Mexico—was more than just a lawyer. Von Hintze called him "a professional attorney for revolutions organized from the United States in Latin America." See Hintze to Bethmann-Hollweg, Feb. 16, 1912, Deutsches Zentralarchiv Potsdam, AA2, no. 4461. In April 1914, a break-in, probably organized by Huerta adherents, took place in Hopkins's Washington office. Some of the letters thus obtained were published in the New York *Herald* in June 1914. They indicate Pierce's strong sympathies for Carranza. Pierce, who was involved not only in the oil business but also in Mexican railroads, had lost much of his power to the British Cowdray interests. As a result of Carranza's control of much of Mexico, both he and Hopkins expressed the hope that he could now regain much of his former influence. He stated his strong support for Carranza's nomination of Alberto Pani as administrator of the Mexican railroads, as well as his disappointment over Villa's reluctance to recognize Pani's authority. See the New York *Herald*, June 28, 1919. The authenticity of the *Herald*'s revelations is confirmed in a telegram sent by Felix Sommerfeld, a close collaborator of Hopkins, to Lazaro de la Garza, Villa's representative. Sommerfeld to de la Garza, undated, University of Texas at Austin, Lazaro de la Garza Collection, wallet no. 13.

⁹ See, especially, Clendenen, *The U.S. and Pancho Villa*; and Arthur S. Link, *Wilson: The Struggle for Neutrality* (Princeton, 1960), chaps. 8, 14.

impose control over all of Mexico.¹⁰ For a time President Wilson, Secretary of State William Jennings Bryan, and a number of high officials in the administration believed they had found such a man in Villa. His public statements bespoke desire for reform, cordiality toward U.S. representatives, belief in free institutions, respect for U.S. property, and commitment to free enterprise. In addition, he had the appearance of a strongman, well able to seize and hold power in Mexico and ready to turn it over to an elected president once the Revolution had triumphed.¹¹

The Wilson administration's sympathetic attitude toward Villa manifested itself clearly in the comment made about Villa by an unnamed high official to the French ambassador in Washington as early as January 1914:

In contrast to what is generally stated, Villa is not a man without property. His parents owned a ranch and enjoyed a certain prosperity. He had no other schooling than primary school but he did attend that. He is not illiterate, as the newspapers describe him to be. His letters are even well formulated. He is of Indian origin, as Huerta is, an excellent rider, and a first-class marksman. He has no fear of physical danger or of the law, and he very early led the life of a ranchman. It is the same life which many people led in remote territories of our own West, territories where authorities had no control, where everyone was his own master, and sometimes imposed his rule on others, controlling his adherents, creating his own code of law.

Villa becomes popular very easily and is able to maintain his popularity. He takes care of his soldiers, helps them, and is very popular among them He would be incapable of ruling, but, if he wanted to, he could very well re-establish order. Were I the president of Mexico, I would charge him with this task: He would successfully carry it out, I am convinced of that; he would force all revolutionaries to maintain order. In the present state of Mexico, I do not see anyone except him who would successfully carry out this task.¹²

The lack of complaints among the large U.S. interests located in Villa's territory and Villa's unwillingness to protest, as Carranza had done, against the U.S. occupation of Veracruz further strengthened the Wilson administration's sympathy for the Chihuahua revolutionary.

Villa's good relationship with U.S. business interests began to deteriorate, however, after his break with Carranza. This diminution of support emerged quite clearly in conversations he held with Duval West, Wilson's special emissary to Mexico, the man charged with making policy recommendations to the U.S. president. West reported that Villa,

on being questioned as to what extent foreigners would be encouraged to develop the country, stated that there would be no disposition to prohibit such development,

¹⁰ For an excellent assessment of Woodrow Wilson's attitude toward underdeveloped countries, see Robert Freeman Smith, *The United States and Revolutionary Nationalism in Mexico* (Chicago, 1971), chap. 2.

¹¹ See Link, *Wilson: The Struggle for Neutrality*, 238–41. Wilson also expressed such ideas in a conversation on the subject of Villa with the French chargé d'affaires in Washington on July 27. The U.S. president said that Villa, upon entering Mexico City at the head of his troops, had declared that he would tolerate no excesses, and Wilson thought him capable of keeping his word. When the French diplomat asked him whom he would support in case of a break between Villa and Carranza, Wilson did not answer him directly. He only stated that he considered Villa's army the essential force of the Revolution and believed that its leader did not aspire to become a candidate for provisional president. See Chargé d'Affaires (Washington) to Foreign Ministry, July 27, 1914, Archives du Ministère de Affaires Étrangères, new ser. (Paris), Mexique Pol. 9.

¹² Jusserand to Doumergues, Jan. 27, 1914, Archives du Ministère de Affaires Étrangères, new ser. (Paris), Mexique Pol. 9.

except that, in the case of lands, foreigners should not, or would not, be permitted to own lands. That it was his idea that the country should be developed by Mexican capital and that this capital should be compelled or required—he did not say what or how or when—to employ itself in the establishment of the usual industrial enterprise.

I get the idea from the foregoing statement and from the failure of General Villa to take the opportunity afforded by the question to make clear the wish of his followers to encourage foreign capital that he is standing on the popular demand that “Mexico should be for Mexicans” and that an open door to foreign investors is an ultimate danger to the nation.¹³

It seems surprising that Villa, whose intelligence was attested to by friend and foe alike, chose to make such a declaration to the man on whom Wilson relied for a critical judgment concerning further U.S. support for Villa. His motive was certainly not propaganda. Villa’s statements were not designed for public consumption; they were never published. Perhaps by this time Villa simply felt strong enough to reveal concepts and sentiments he had not dared to express before. When he made the declaration in February 1915, his decisive defeat at Celaya was still two months off, and Villa, like many observers in Mexico, was convinced that Carranza’s defeat was imminent.

Whatever the cause, Villa’s remarks happened to coincide with his increased pressure on U.S. business interests in Mexico. Financial problems had already begun to plague him by the end of 1914. He had exhausted a great part of the resources—mostly cotton and cattle—that the confiscated estates had brought him. As the civil war with Carranza wore on and escalated, many U.S. enterprises, mainly the mining companies, suspended operations, further diminishing the flow of revenue. Villa was more acutely affected by such suspensions than Carranza. Many oil companies from which Carranza drew his revenue were located near the coast and felt sufficiently well protected by foreign warships even to expand their operations. To compound the evils besetting Villa by the end of 1914, his access to weapons was obstructed by the outbreak of the First World War. Suddenly, what had been a “buyer’s market” turned into a “seller’s market,” and Villa had to compete with Great Britain, France, and other powers for scarce U.S. arms. No wonder, then, that he should attempt to relieve his financial distress somewhat by imposing heavier taxes on U.S. companies and by trying to cajole them into resuming operations through threats of confiscation.¹⁴

After his defeat at Celaya in April 1915, Villa’s financial situation worsened. Mexican and U.S. speculators, as well as large U.S. companies, began to divest themselves of large amounts of Villa currency acquired when it seemed that he would soon prevail.¹⁵ The effects were immediately obvious: prices soared, food became scarce, and food riots broke out all over the Villa zone.¹⁶

¹³ Undated Report by Duval West to the Secretary of State, National Archives, Dept. of State Files, Record Group 59, File 812.00 14622.

¹⁴ Marvin D. Bernstein, *The Mexican Mining Industry, 1890–1950* (New York, 1964), chap. 9.

¹⁵ Report by agent Emilio Zapico to the Spanish ambassador in Washington, December 1915, Archives of the Spanish Foreign Ministry, as quoted in Vicente Gonzalez Loscertales, “Los Españoles en la Vida Social, Política y Económica de Méjico, 1910–1930” (Ph.D. dissertation, University of Madrid, 1976).

¹⁶ Francisco Almada, *Historia de la Revolución en el Estado de Chihuahua*, 2 (Mexico, 1971): 236–38, and *Historia de la Revolución en el Estado de Sonora* (Mexico, 1971), 172–75. See the reports of special Spanish agent Emilio Zapico to the Spanish ambassador in Washington, August 1915, in Loscertales, “Los Españoles en la Vida Social, Política y Económica de Méjico, 1910–1930,” 293–395.

At this moment Villa suffered his most palpable loss of popular support. Nothing Carranza ever did in the way of promising social and political reform proved as detrimental to Villa as the economic collapse that struck the territories under his control. Villa sought relief by increasing the financial pressure on U.S. companies, which in turn provoked angry reprimands from the U.S. state department but not a complete break between Villa and the Wilson administration. Both sides still needed each other to forestall a complete victory by Carranza. Villa agreed to the administration's proposal that he send delegates to participate in a peace conference—sponsored by the ABC powers and the United States—at which the contending Mexican factions would be represented.¹⁷

From May 1915 onward, after Villa's military defeats had decisively if not fatally weakened his hold on Mexico, his relations with Washington grew more and more muddled. Tensions between Villa and U.S. companies mounted. Wilson tried to steer a precarious course, cooling his support of Villa appreciably though not entirely. He wished to avert a complete takeover by Carranza. Attempts were made to impose a solution to the civil war by eliminating both Villa and Carranza from the scene. In October 1915 Wilson finally recognized Carranza's government. U.S. fears of German intrigues in Mexico significantly influenced this decision.

The history of Mexican-American relations between May and October 1915 is a topic well beyond the scope of this paper, all the more so since the nature of these relations has been closely examined elsewhere.¹⁸ What has not been sufficiently analyzed, and is of relevance to this study, is the suddenness of Wilson's change of heart regarding Carranza. Only a few weeks before the United States government granted recognition to the Carranza government, Wilson sent General Hugh Scott to negotiate with Villa concerning the return of some expropriated U.S. holdings. Lansing told Scott to assure Villa that Wilson would under no circumstances recognize Carranza. Scott later stated that he never delivered this message to Villa, but Villa's agents in the United States did, in fact, learn of Wilson's assurances.¹⁹ The president's sudden about-face, coming only a short time after he had invited Villa to send delegates to Washington to participate in a peace conference and after Lansing had given assurances that the United States would never recognize Carranza, made Villa suspicious that some important clandestine event had changed Wilson's mind.

¹⁷ Clendenen, *The U.S. and Pancho Villa*, 165–91.

¹⁸ See, especially, Link, *Wilson: The Struggle for Neutrality*, chap. 14, 456–94; and Clendenen, *The U.S. and Pancho Villa*, 155–207.

¹⁹ In a letter of October 14, 1915 to James R. Garfield, who was acting as a lobbyist for Villa, U.S. Chief of Staff General Hugh Scott wrote, "You are not the only one astounded by the action of the administration. Nobody in the State Department, below the Secretary himself, can understand it. You remember I told you that Mr. Lansing told me to say to Villa that under no circumstances would we recognize Carranza. I had a lucid interval down there while talking to Villa, and did not tell him what Secretary Lansing told me to tell him, as I believe that matters of that kind should be held back when dealing with primitive people unless you have the carrying out of them in your own hands, otherwise you can't explain to them that it was not your fault if not carried out." Library of Congress, Scott Papers, box 20 (general correspondence).

VILLA'S FIRST REACTION TO THE RECOGNITION of his enemy was mild. He made no overt declarations against the Wilson administration, and no Americans in his zone suffered harm. In fact, he still hoped to circumvent the most painful consequence of Wilson's action—the embargo on weapons—by carrying out his planned invasion of the border state of Sonora. The state was wracked by a civil war between the forces of Governor José Maria Maytorena, who had allied himself with Villa, and those loyal to Carranza. By invading Sonora, Villa hoped to tip the scale and assume control of the entire state. Domination of both Chihuahua and Sonora would have diminished significantly the effectiveness of the weapons embargo. It would have meant, as one U.S. military intelligence officer put it, “the increased possibility of smuggling arms from the United States.”²⁰ It would also have placed appreciable amounts of U.S. property under Villa's control. Perhaps the administration in Washington, even though it had already recognized Carranza, would be forced to accommodate Villa in some fashion.

This hope is probably one of the main reasons why Roque González Garza, former head of the Government of the Revolutionary Convention and Villa's representative in the United States, urged his chief in October 1915, after the recognition of Carranza by the United States, to proceed as rapidly as possible with plans to attack Carranza's forces at Agua Prieta, Sonora.²¹ The Sonoran campaign, however, ended disastrously for Villa. His army was decimated when Carranza unexpectedly reinforced the garrison at Agua Prieta by marching his troops across U.S. territory—with official U.S. permission.²² After this humiliating defeat, Villa assumed a new attitude toward the United States, one that was to dominate his actions more and more between November 1915 and March 1916, when his troops attacked Columbus, New Mexico. This new attitude doubtlessly owed much to the interpretation that Roque González Garza gave to Wilson's decision to recognize Carranza. On October 29 the emissary wrote Villa a long description and analysis of the events leading up to the recognition of his enemy:

It was a great blow to me to see that you have always been miserably deceived; possibly this took place in good faith but you were always deceived. I was also deceived After arriving in Torreon . . . I was clearly told that, from the point of view of international political relations, our situation was very good; we were one step from recognition by the United States A few days went by and you received the clearest assurances that, from the point of view of international politics, everything was proceeding in your favor; that only a small effort on our part was required for the U.S. government to take us into consideration and that the original plan of the participants at the conference would be implemented with satisfactory results for us.

²⁰ Report of Operations of “General” Francisco Villa since November 1915, Headquarters Punitive Expedition, In the Field, Mexico, July 31, 1916, National Archives, Mexican Claims Commission, 3. For a detailed description and analysis of Villa's Sonoran campaign, see Thomas H. Naylor, “Massacre at San Pedro de la Cueva: The Significance of Pancho Villa's Disastrous Sonora Campaign,” *Western Historical Quarterly*, 8 (1977): 125–50.

²¹ Roque González Garza to Villa, Oct. 26, 1915, private archive of General Roque González Garza (Mexico City). I wish to express my thanks to General González Garza's daughter, Lourdes, for permission to use this archive.

²² Naylor, “Massacre at San Pedro de la Cueva,” 130.

González Garza did not mention the name of the person who had given these assurances to Villa. That he left open the possibility of the intermediary's good faith indicates that he was probably referring to George Carothers, the U.S. special agent in the Villa camp with whom Villa had entertained good relations.

Bitterly, González Garza then went on to describe how Villa's delegates at the Washington peace conference were treated by their American hosts:

Our situation was depressing. Everything turned out to have been a lie; we were very badly off; we were not even listened to The 9th of October arrived and the participants at the conference decided to recognize Carranza This decision, communicated *ex abrupto* to the four winds, was an enormous humiliation for us since we were delegates to the peace conference. We were not told anything and the solemn declarations made by Wilson at an earlier date were simply discarded. All historical precedents were ignored. Even common sense was not respected, since we had come to the conference ready to make peace but in an honorable way. This resolution was approved and we suffered a great blow.

He continued angrily,

I have seen many injustices, but I have never thought that Carranza would triumph in the international political field after he played the comedy of being the most nationalistic of all Mexicans and after he provoked the United States two or three times. I do not entirely know what has been decided concretely, but I am convinced that something very dark has been agreed on; for I have no other explanation for the sudden change in U.S. policy against our group and in favor of Carranza.

In another part of the letter, he stated, "God knows how many secret pacts" Carranza had signed with the United States.²³

The dark plot and secret pact that he intimated were spelled out in a manifesto issued on November 5, 1915 in Naco, Sonora, signed by Villa and probably drafted by Roque González Garza's brother Federico, who for a long time had been part of Villa's administration in the state of Chihuahua.²⁴ It was published in the November 21 issue of Villa's newspaper, *Vida Nueva*. The manifesto raised the question why Carranza—who "had never given guarantees to Americans, who had plundered them, who had deprived foreigners as often as he could of the lands they owned in the eastern and southern parts of the Republic, and who had always aroused the repugnance of the U.S."—had suddenly obtained not only the recognition but also the active support of the United States. According to Villa, U.S. support to Carranza entailed nothing less than a \$500-million loan and permission for Carranza's troops to cross into U.S. territory. The manifesto bluntly answered its own question: "The price for these favors was simply the sale of our country by the traitor Carranza."

The manifesto further charged that Carranza had agreed to eight conditions imposed by the United States: (1) amnesty to all political prisoners; (2)

²³ González Garza to Villa, Oct. 26, 1915, González Garza archive.

²⁴ Almada, *Historia de la Revolución en el Estado de Chihuahua*, 2: 382. Almada quotes a conversation with Villa's former secretary of state, Silvestre Terrazas, as the basis for his observation on the authorship of the manifesto.

a ninety-nine-year concession granting the United States rights over Magdalena Bay, Tehuantepec, and an unnamed region in the oil zone; (3) an agreement that the ministries of the interior, foreign affairs, and finance would be filled by candidates enjoying the support of the Washington government; (4) all paper money issued by the Revolution would be consolidated after consultation with a representative named by the White House; (5) all just claims by foreigners for damages caused by the Revolution would be paid and all confiscated property returned; (6) the Mexican National Railways would be controlled by the governing board in New York until the debts to this board were repaid; (7) the United States, through Wall Street bankers, would grant a \$500-million loan to the Mexican government to be guaranteed by a lien on the entire income of the Mexican treasury, with a representative of the U.S. government to have supervision over Mexico's compliance with this provision; and (8) General Pablo Gonzalez would be named provisional president and would call for elections within six months.

Villa's policies in the next months were clearly presaged in passages of this manifesto. "Can foreigners, especially the Yanquis," Villa asked, "harbor the illusion that they can exploit 'peacefully while thanking God' the riches of Mexican soil?" He continued,

Can they be naive enough to assume that Carranza's government can give them effective guarantees? . . . As far as I am concerned, I sincerely and emphatically declare that I have much to thank Mr. Wilson for, since he has freed me of the obligation to give guarantees to foreigners and, above all, to those who were once free citizens and are now vassals of an evangelizing professor of philosophy who is destroying the independence of a friendly people and who violates the sovereignty of the states of Arizona and Texas, allowing their soil to be crossed by the "constitutional troops." This does not imply a feeling of enmity or hatred against the real people of the United States of North America, whom I respect and admire for their glorious traditions, for their example of order and economy, and for their love of progress.

Villa raised the possibility of an armed conflict with the United States, while he denied that that was his intent:

After such a clear-cut declaration, I wish to state that I have no motive for wishing a conflict between my country and the United States. For this reason, after all I have said, I decline any responsibility for future events, since the American people know perfectly well that I have always carried out superhuman efforts to give guarantees to each of their nationals who is living in our country. Let history assign responsibilities.²⁵

Significantly, not one of the innumerable political, diplomatic, military, or financial agents the United States had stationed along the border reported the publication of this manifesto which contained such clear forebodings of Villa's intentions. Not even George Carothers, the former U.S. special agent with Villa, who was charged by the U.S. state department with assembling all possible information on Villa's activities, made any mention of the Naco Manifesto. Did these men simply not bother to read *Vida Nueva*, or had they

²⁵ *Vida Nueva* (Chihuahua), Nov. 21, 1915. Although it has been ignored by U.S. historians, Almada printed this manifesto in the appendix to his *La Revolución en el Estado de Chihuahua*, 2: 382.

become captives of their own image of Villa the Bandit, who, as events began to turn against him, would leave Mexico to enjoy elsewhere the enormous sums of money they believed he had accumulated while in power? Did Leon Canova, the head of the Latin American desk in Washington, know of the manifesto and decide, for reasons of his own, to suppress it?²⁶

On November 22, 1915, after his troops had launched an unsuccessful attack on the Carranza garrison at Hermosillo, capital of the state of Sonora, Villa sent a letter to the two commanders of Carranza's troops in that city—Manuel Dieguez and Angel Flores. In it he mentioned the eight provisions of the secret pact that, in his opinion, Carranza had signed with the United States and stated that “we are now in the hands of the North Americans; we have accepted a Yanqui protectorate.” He added that Carranza had now converted the conventionist movement (Villa and his allies) into the only group that defended the integrity and independence of Mexico, and, for this reason and in spite of all defeats, its triumph was now inevitable. That the United States allowed Carranza's troops to cross its territory meant that, “when the United States wants or needs to, it can enter Mexican territory. Will you allow this?” Villa asked Carranza's generals. He made no specific overtures to them; he only asked them to give their opinions on the charges. Villa, most likely, hoped to enter into some kind of negotiations with Carranza's commanders. But, although Angel Flores did send a reply to Villa (the contents of which are not known), Dieguez declined to do even that.²⁷

Villa's hope that the accusations against Carranza would stave off disaster in his Sonoran campaign proved unfounded. Three weeks after his unsuccessful overtures to the two Carranza generals, Villa returned to Chihuahua at the head of a decimated and demoralized army and found Carranza's troops gaining on his last strongholds in the state. As disaster approached, the specter of a pact between Wilson and Carranza began to loom ever larger in Villa's thinking. On December 16 he sent a letter to the commanders of the Carranza forces heading for Chihuahua, who had by now reached the city of Camargo. In it he made far more concrete proposals than before. After iterating his accusations against Carranza, Villa stated that, because of this new development, his troops had stopped fighting Carranzistas “so as not to shed Mexican blood.” To the Carranza generals he proposed an alliance “that would unite all of us against the Yanqui who, because of racial antagonisms and commercial and economic ambitions, is the natural enemy of our race and of all Latin countries.” In case such an alliance was signed, he wrote, he would give up command of his troops.²⁸ Three days after signing this letter, Villa made a farewell speech to the people of Chihuahua from the balcony of

²⁶ Since, as I contend, Villa's charges were based on a plot involving Canova, the latter might have wanted to prevent Villa's accusations from being brought to the attention of his superiors. There is, however, no evidence to substantiate this assumption.

²⁷ Alberto Calzadiaz Barrera, *Hechos Reales de la Revolución*, 3 (Mexico, 1972): 141–43.

²⁸ Francisco Villa to Jefe de la Columna Expedicionaria del Norte y a los demas Generales que forman parte de ella: Camargo o en donde se Encuentran, Dec. 16, 1915, University of California at Berkeley, Bancroft Library, Silvestre Terrazas Papers, vol. 78, pt. 1.



Villa in the mountains. From Jessie Peterson and Thelma Cox Knoles, eds., *Pancho Villa: Intimate Recollections by People Who Knew Him* (New York, 1977). Photo courtesy Hastings House Publishers.

the Municipal Palace. He repeated his accusations that Carranza had signed a secret pact with the United States.²⁹

As Villa's forces dwindled and the occupation of all cities in Chihuahua by Carranza's forces became imminent, many U.S. observers expected Villa to cross the border to find refuge in the United States.³⁰ This pattern of behavior had proved characteristic of many Latin American *caudillos* who left their countries after unsuccessful coups and revolts to reward themselves for their exertions with whatever wealth they had been able to accrue while they were masters of the national treasury. With no one restraining or supervising him, Villa had controlled the finances of the Division of the North for years. Had he wanted, he could easily have deposited large sums of money in foreign banks. But Villa did not conform to the model.

A few days after his farewell speech, Villa went to the railroad station of the capital city of the state to bid goodbye to Silvestre Terrazas, his close collaborator and Chihuahuan secretary of state. He instructed Terrazas to go to El Paso to arrange with Carranza's authorities for the surrender of Villa's troops at the border town of Ciudad Juárez. Terrazas suggested to Villa that he leave Mexico, perhaps in order to go to Europe and study the new military techniques being utilized in World War I. Villa answered that he would instead retreat with a number of loyal men into the mountains, where he could easily elude any hostile troops. "I will never leave my country," he announced. "Here I will stay and fight." But he still had hopes for change in Mexico. "Before six months have passed," he told Terrazas, "it will become clear that U.S. recognition of the Carranza faction was not disinterested but in fact dependent on the same proposals which Washington made as the price of granting me recognition, proposals which I rejected."³¹

Villa did not reveal to Terrazas the meaning of his six months' timetable. But he did make his plans abundantly clear in a letter, written a few days later, to Emiliano Zapata, which was found on the body of a dead Villista after the Columbus attack. After attributing responsibility for his defeat at Sonora to Wilson for allowing Carranza's troops to cross U.S. territory and after describing the secret agreement contained in his Naco Manifesto, Villa wrote to his ally,

From the foregoing you will see that the sale of this country is complete and under these circumstances, for reasons stated previously, we have decided not to fire a bullet more against Mexicans, our brothers, and to prepare and organize ourselves to attack the Americans in their own dens and make them know that Mexico is a land for the free and a tomb for thrones, crowns, and traitors.

With the aim of informing the people of the situation and in order to organize and recruit the greatest possible number of men with the aforementioned aim, I have divided my army into guerrilla bands, and each chief will go to that part of the country he considers appropriate for a period of six months. That is the time period we

²⁹ Report of Operations of "General" Francisco Villa, 5.

³⁰ Clendenen, *The U.S. and Pancho Villa*, 222-23. Fidel Avila, Villa's Governor of Chihuahua, reinforced this interpretation by sending Wilson a telegram that indicated that Villa would seek refuge in the United States; *Papers Relating to the Foreign Relations of the United States* (1915), 777.

³¹ Silvestre Terrazas, "El Verdadero Pancho Villa" (chap. 53 of Terrazas's *Memoirs*), in *Boletín de la Sociedad Chihuahuense de Estudios Históricos*, 8 (1955): 769-71.

have set to meet in the state of Chihuahua with all the forces we will have recruited in the country to carry out the movement that will unite all Mexicans.

The move we have to make to the United States can only be accomplished through the north, as we do not have any ships. I beg you to tell me if you agree to come here with all your troops and on what date so that I may have the pleasure to go personally to meet you and together start the work of reconstruction and enhancement of Mexico, punishing our eternal enemy, the one that has always been encouraging hatred and provoking difficulties and quarrels among our race.³²

The letter to Zapata was preceded by a conference of all military chiefs still loyal to Villa at the Hacienda de Bustillos on December 23, 1915. The conference adopted a resolution that stated,

Considering: that the C. VENUSTIANO CARRANZA as Chief of the opposing party has contracted compromises which place the country in the hands of foreigners, a thing the honorable Mexican people will never approve.

Considering: that the United States will exact the fulfillment of said compromises and that upon not obtaining same will intervene in our country under any pretext, . . . waiting but for the hour of our utmost weakness!

Considering: that rather than permit the hour to come when we would fall into the hands of the ambitious North Americans, we would become the accomplices to the traitor's party, who has looked for the ruin of our country.

In the meeting held today we have agreed that General FRANCISCO VILLA, Chief S. of C. of the conventional army, shall take the proper steps to advise the nation of the threatening danger to its integrity, and we shall proceed to organize in the respective regions of the country.³³

On the same day that Villa appealed to Zapata, he sent emissaries to mobilize troops in other parts of the country to fight against the United States.³⁴ It is significant that none of Villa's upper- or middle-class supporters—such as Felipe Angeles, Raul Madero, José Maria Maytorena, or even the González Garza brothers—were involved in this scheme.

While preparing his attack on the United States, Villa decided to implement the warning given the U.S. government and U.S. interests in his Naco Manifesto, in which he had stated that Americans should not believe that Carranza was capable of giving them sufficient guarantees to continue exploiting the resources of Mexico. Villa began to confiscate U.S. property wherever he found it. In January 1916 he occupied Babicora, William Randolph Hearst's ranch, which had long been spared, and confiscated a large number of cattle and horses. In Santa Isabel on January 17 loyal Villa troops under the command of Pablo Lopez stopped a train carrying seventeen American mining engineers who were returning to Mexico from the United States to restore operations in a U.S.-owned mine. All were executed.³⁵

³² This letter was part of a collection of documents found on a dead Villista after the Columbus attack. They never reached the state department files but are contained in the Adjutant General's Office, File 2384662, Record Group 94, along with File 2377632. The complete text of this letter was first published in E. Bruce White, "The Muddled Waters of Columbus, New Mexico," 72-92. A complete list of the documents and an attempt to analyze them was published at the same time in Harris and Sadler, "Pancho Villa and the Columbus Raid: The Missing Documents," 345-47.

³³ See Adjutant General's Office, File 2384662.

³⁴ *Ibid.*

³⁵ Investigators of the Punitive Expedition concluded in July 1916, "In this connection it will be observed



Villistas killed in the Columbus raid. From Peterson and Knoles, eds., *Pancho Villa* (New York, 1977). Photo courtesy Hastings House Publishers.

For reasons that are not clear, Villa decided to strike directly at the United States far earlier than he had announced in his letter to Zapata. According to one participant in the attack, Juan Caballero, Villa concentrated a large part of his troops at the Hacienda de San Jeronimo and told them he planned to attack the United States. Again, the main reason he gave was the secret U.S.-Carranza pact. Villa told them that the pact had been offered to him earlier at Guadalajara, but he had refused to sign it.³⁶ Caballero reported that Villa first decided to attack Presidio, Texas, but changed his plan after part of his forces deserted. He was afraid they might reveal his plans to the Americans. Thereafter, Villa became extremely reserved about the name of the town he planned

that Villa's location at the time of the massacre was far from the scene where the act was enacted and that he was so situated and the circumstances attending the accidental arrival of the train bearing the ill-fated seventeen Americans, were such that Villa could not have had first-hand advance information to have been able to issue direct orders to Lopez to kill them. There is reason to believe, however, that orders to rid Mexico were given Lopez by Villa, either by telegraphic instructions through Rodriguez before Lopez left Casas Grandes for Madera, or later in person at the unexpected meeting at El Valle between Villa and Rodriguez. Orders from Villa to kill Americans, could have reached Lopez at any time, of course, but it can be stated quite positively that Villa was not in the vicinity of Santa Ysabel at the time of the massacre, and further that when reports of the incident reached him by couriers he was inclined to deny its authenticity." Report of Operations of "General" Francisco Villa, 8. Nevertheless, this killing was definitely congruent with the warnings Villa clearly expressed to Wilson and to American mining companies in his Naco Manifesto.

³⁶ Confederación de Veteranos Revolucionarios de la División del Norte, Relato de Hechos Históricos de la Actuación del Gral. Francisco Villa y sus Tropas, Archivo de la Palabra México, Typescript by Juan Caballero (Dec. 7, 1971).

to attack. It was only when they arrived near Columbus, New Mexico, that his men found out where they were going.

Why Villa picked Columbus for his target is not apparent. He might have wanted to use the attack to settle old scores. Perhaps he wanted to take his revenge on the arms dealer, Sam Ravel, who he felt had betrayed him, although Caballero doubts this version. Perhaps he hoped to get money and supplies there.³⁷ But such considerations were clearly secondary; none constituted his main motive for launching an attack on the United States—his firm conviction that Carranza had sold out Mexico to the United States.

WHAT REAL SUBSTANCE WAS THERE to Villa's profound belief in the existence of a pact between Carranza and the United States? Had such a pact in fact ever been proposed by Wilson? Had it been hatched by members of his administration? Was it pure invention by Villa or his collaborators? Was it a scheme devised by U.S. business interests, which hoped Villa's actions would provoke U.S. intervention in Mexico? Was it a concoction of the German secret service, which hoped to distract U.S. attention from the European theater of war by forcing it to intervene in Mexico?

There is not the slightest evidence that Carranza ever signed such a pact. Of all the accusations Villa leveled against Carranza, only one—point five of the eight-point manifesto—contained a grain of truth. Carranza had indeed agreed to examine U.S. claims for damages suffered during the Revolution and was returning confiscated properties to their former owners.³⁸ This, no doubt, was a major victory for the conservative forces. It was not, however, the result of U.S. pressure but of Carranza's own conservative convictions, to which he had held fast from the day he joined the Revolution. His readiness to negotiate U.S. claims could scarcely justify the accusation that he had converted Mexico into a U.S. protectorate. Such a pact would have been entirely incompatible with his staunch nationalistic posture. Nor is there the slightest proof that Woodrow Wilson either proposed or contemplated such an agreement with any Mexican faction at any time during the Revolution.

The pact, nevertheless, was not entirely a figment of Villa's imagination; in fact, documents show that in 1915 such a plan had been conceived and was seriously considered by a leading official of the state department, representatives of U.S. business interests, and Mexican conservatives (but not of Carranza's faction). In May of that year, Leon Canova, head of the state department's Mexican desk, and Eduardo Iturbide, who had been Mexico City's chief of police during the administration of Victoriano Huerta, elaborated a scheme for a U.S.-supported counterrevolution in Mexico. They hoped to involve at least part of Villa's army at a time when Villa had suffered some serious defeats but Carranza's supremacy had not yet been clearly established.

³⁷ *Ibid.*

³⁸ There is no evidence that Carranza ever went beyond the limited concessions he made publicly. *Papers Relating to the Foreign Relations of the United States* (1915), 795–97.

Wilson hoped to eliminate both Mexican leaders from the political scene by recognizing and helping a candidate who could gain overwhelming support from both factions and thus might be able to end the civil strife in Mexico. At that point, Canova proposed to a number of administration officials that the United States throw its support to a counterrevolutionary group headed by Iturbide, openly avow that support, and supply that group with stocks of food which it could then distribute among the population. These gestures, it was hoped, would secure for Iturbide the popularity he lacked.³⁹ In return for such help the conservatives were to grant wide-ranging rights to both the U.S. government and U.S. bankers, including "American supervision of customs collection," in exchange for a large loan—\$500 million was mentioned—from U.S. banks. The conservatives were also to accept the appointment by the United States of an "unofficial administrative advisor" with unspecified powers to "oversee the necessary reforms."⁴⁰ In his memorandum to then Secretary of State Bryan, Canova did not explain what he meant by "necessary reforms," but he did stipulate in another memorandum sent to Chandler Anderson, who frequently served as intermediary between the administration and business interests that "all Church and other real property confiscated by revolutionary bands or others without proper or due process of law since February 13, 1913 shall be re-occupied by their legal owners."⁴¹

Canova's plot was far more than an attempt by a high state department official and a few Mexican and American associates to secure advantages in Mexico. The plan was backed by important segments of Mexico's pre-revolutionary oligarchy, of whom Manuel Calero was a representative, and by U.S. business interests, for whom Chandler Anderson was spokesman.⁴² Its purpose was to exploit disunion within the revolutionary camp by re-establishing a Díaz-like regime that, unlike its real predecessor, would be dominated by the United States. The plan also had vocal support within the administration; most outspoken was Secretary of the Interior Franklin K. Lane, who in later years became closely identified with oil interests.⁴³ On his initiative the plan was discussed at a cabinet meeting. But Bryan dismissed the plan, declaring that the United States "should not take up a man who

³⁹ For a description of the Canova-Iturbide conspiracy, see Link, *Wilson: The Struggle for Neutrality*, 470–74.

⁴⁰ Canova to the Secretary of State, Dept. of State Files, RG 59, File 812.00 15531 1/2.

⁴¹ Diary of Chandler Anderson, May 28, 1915, Library of Congress. The state department papers contain only the barest outline of Canova's plot. Most of the available information is contained in Anderson's diary, especially the entries for April 23, May 14, May 19, May 28, June 1, June 29, July 23, and July 30, 1915. Very apparent is the conservatives' desire to apply a strategy in 1915 similar to that they applied in 1911. They were willing to agree to some "compromises" as far as the composition of the government was concerned. Iturbide was quite willing, for example, to include Manuel Bonilla as a representative of the pro-Villa forces and Alvaro Obregón as a representative of the pro-Carranza forces. To ensure conservative control of the Mexican army similar to that in 1911, "Iturbide himself would have no part in the new Government, but would act as the leader of the military forces, supporting it, which he regarded as essential, in order that he might be in a position to compel the new government to carry out the pledges which it would have to make in order to secure the support of the United States." Anderson Diary, July 22, 1915.

⁴² For Anderson's role as lobbyist for American mining, oil, and other interests, see Smith, *The U.S. and Revolutionary Nationalism*, 95.

⁴³ *Ibid.*

would probably play in with the reactionaries.”⁴⁴ Although Wilson did not voice any opinion at these meetings, he later expressed agreement with Bryan’s position, and Canova’s plan was discarded.⁴⁵

The plan contained three of the main provisions to which, according to Villa’s accusations, Carranza and the United States had agreed: (1) the \$500-million loan by U.S. bankers to Mexico and American financial control of essential parts of Mexico’s economy, (2) strong influence over the Mexican government by advisers in Washington, and (3) the return of expropriated holdings to foreigners and Mexican enemies of the Revolution. In their talks with U.S. officials, the plotters never mentioned other provisions that appeared in Villa’s Naco Manifesto, provisions that would have made Mexico not only economically but militarily dependent upon the United States: U.S. naval bases in Magdalena Bay; U.S. control of the Isthmus to Tehuantepec, the oil regions, and the Mexican railways; and the right of the U.S. government to send troops to Mexico whenever it considered such a step to be necessary. Nor did Canova and Iturbide mention the right of the United States to impose its candidates as secretaries of foreign relations and of finance on a new Mexican government.

Was there any validity, then, to the other charges of the Naco Manifesto? There is a very strong possibility that the Canova-Iturbide plot went far beyond what they were willing to reveal to U.S. authorities and that it included a covert pact encompassing most of Villa’s charges. In their proposals to U.S. officials, the plotters had remained vague on three counts: what financial interests backed their plan; what they had promised these interests in return; and what the specific tasks and powers of the U.S. advisers were to be. In his letter to Bryan, Canova made only one reference to financial support. He included letters from Speyer and Company offering to lend \$500 million to the new Mexican government, if the United States were to go along with Canova’s plan. Iturbide was somewhat franker in a conversation with Postmaster General Albert S. Burleson, according to a memorandum by Anderson:

Burleson . . . stated that he thought the administration would favor his [Iturbide’s] movement, if it could show sufficient strength and backing, and that it was clean. This last seemed to offend Iturbide somewhat because he felt that his own connection with it was a guarantee of its cleanness. It appeared that what Burleson wanted to know was who was backing it financially and what, if any, obligations they had undertaken. Iturbide told him that there were three bankers who had offered to finance the movement. One was Mr. Williams, the brother of John Skelton Williams; the other was Mr. Kenna, who was vouched for by Senator Underwood, and another whose name he said frankly that he did not know because the offer had come in an indirect way. He said further that Speyer and Company were prepared to finance the movement if it were endorsed by the administration.⁴⁶

⁴⁴ As quoted in David F. Houston, *Eight Years with Wilson’s Cabinet, 1913 to 1920*, 1 (New York, 1926): 133.

⁴⁵ Link, *Wilson: The Struggle for Neutrality*, 475–76.

⁴⁶ Diary of Chandler Anderson, July 23, 1915, Library of Congress. Robert Lancaster Williams was a banker and railroad executive, whose brother, John Skelton Williams, had been appointed comptroller of the currency by Woodrow Wilson. Edward Dudley Kenna was also a railway executive, who had been vouched for by a Democratic senator.

That the third backer chose not to reveal his identity or that Iturbide chose not to disclose his identity may indicate that the person was judged unacceptable to either Bryan or the U.S. government. Significantly, Iturbide completely evaded the question as to what obligations his party had assumed toward U.S. special interests. Naturally, the intriguing question about the Canova-Iturbide plot is what was meant by "necessary reforms" and what was to have been the power of the "U.S. unofficial adviser" charged with implementing them. Although we have no direct evidence as to what was intended, we may surmise the outlines based on still other plans and agreements that were drawn up some time after the attack on Columbus.

On December 6, 1917—two and a half years after Canova and Iturbide came out with their first plot concerning Mexico—Secretary of State Lansing wrote a secret memorandum to Wilson concerning yet another plot. This document, which was classified until recently, merits extended quotation.

This afternoon, X came to see me and said that he had run across a most remarkable plot in relation to Mexico. (X is an intimate friend of mine in whom I have implicit confidence.) He had been consulted by a Mexican of good family belonging to the old regime, whom I will call "S." I am personally acquainted with S and believe him to be honorable and straightforward. X said that S stated that he had been approached with a proposition to undertake a revolution against the Mexican government, that the general plan had been worked out, and [that] many prominent Mexicans and Americans in this country were involved and arrangements were made to place ample funds at the disposal of those that were engaged in the enterprise.

S told X that he had been interviewed by Cecil Ira McRaynolds, an attorney from New York, who said he was acting for Anderson Hurd, and that McRaynolds told him that this planned revolution had the approval of the Department of State, that V, a high official in the Department, knew of the plan and had assured him of the approval of the government.

S said that, while it seemed to be alright [*sic*], he did not wish to do anything in the matter unless he was absolutely sure that this government approved it. Knowing that X was a friend of mine, he asked him to see me and find the truth. X told him that he was familiar with my policy in relation to Mexico and that he was certain that V had not been authorized to speak for the Department or to encourage this movement but that he would see me if S desired him to do so. S replied that he did because he would have nothing to do with it unless it was alright. He then gave X a carbon copy on pink paper of a bilingual letter setting forth the arrangements for financing the movement and the concessions and compensation to be given by the new government when it came into power.

Six days later, X again saw Lansing and added more details to the plot:

He [X] said that he was informed that Anderson Hurd, who appeared to be the central figure in the movement, though probably only an agent, had been employed by the Shipping Board to negotiate for the purchase of German ships in Mexican ports, and that Cecil Ira McRaynolds, his attorney, had also been employed by the Board in some capacity. McRaynolds told the Mexican S that he was certain that the Department of State knew all about the plan because, he asserted, the Shipping Board was behind it and that Senator James A. O'Gorman of New York had talked the matter over with Frank L. Polk, a Counselor in the State Department. (Polk told me later that O'Gorman had called to see him about the purchase of some ships in Uruguay but never mentioned Mexico.)

McRaynolds told S that the primary purpose of the proposed revolution was to secure the oil at Tampico and the German ships in Mexican waters; and that he had had a conference at which were present Corwin, Swain, and Helm of the Standard Oil Co., and V of the Department of State. They met in New York and discussed the plan.

McRaynolds also told S the Standard Oil Co. was to put up \$5 million to begin with. Of that sum, \$2.5 million were to be used for purchasing the ships; \$1.5 million to finance the revolution; and the balance—\$1 million—was to go to those who had been instrumental in aiding the movement. S told X that he understood that V was to be paid out of the latter sum.⁴⁷

Obviously, Lansing felt that the matter was too sensitive for him to commit to paper the names of the persons involved, even though the paper was a secret memorandum to the president. Other documents in the same file, however, conclusively identify two of the three persons mentioned. The high official of the Department of State, "V," was Leon Canova, the man originally in charge of Washington's Latin American desk, later solely in charge of Mexican affairs. "S," the Mexican of "good family," was Eduardo Iturbide. After Lansing discovered the plot, he relieved Canova of his post.⁴⁸

The bilingual pact signed by the conspirators, which X (whose identity cannot be ascertained) handed to Lansing, contains provisions strikingly similar to those mentioned by Villa in his accusations against Carranza. Point 10 of this secret agreement stated,

In recognition of the services which you and your principals obligate themselves to perform I do, for myself, my principals, and associates, obligate myself and them to the end that I and they and the political party which sustains us will use all our influence and the means at our disposal to bring about the following: (a) That the appointments of the secretaries of Foreign Relations and of the Treasury of the Mexican government will be given to men especially fitted to re-establish and maintain complete harmony between the governments of Mexico and the United States and inspire confidence in you and your principals with reference to the carrying out of

⁴⁷ Secret Memoranda of Secretary of State Lansing, Dec. 6 and Dec. 12, 1917, National Archives, Dept. of State, Office of the Counselor, Leland Harrison File, box 208 (Mexican intrigue).

⁴⁸ Canova's identity as "V" is confirmed by a document that was included in the file, namely, a wire from the U.S. Embassy in London, which stated, "Admiralty greatly disturbed by a report that Canova has been implicated in some Mexican intrigue and is likely to lose his job." See Harrison to Bell, Jan. 17, 1918, National Archives, Dept. of State, Leland Harrison File, box 208 (Mexican intrigue). Another memorandum in the same file of July 12, 1918 confirmed Iturbide's identity as "S": "Informant told me that Michael Spellacy, an oil man, had told Congressman McLemore of Texas in confidence, and that McLemore had told informant in confidence, and informant told me in confidence, practically the identical information that was in the secret memorandum of Secretary Lansing which was turned over to this office at the beginning of the investigation of Mexican affairs. Informant said that all this came from Iturbide telling Spellacy some time ago that the State Department, through "V," had half sanctioned the financing of a revolution in Mexico through certain New York bankers to the amount of five million dollars, and that it was understood between I and "V" that "V" was to get a portion of this money for helping to put the deal through; that "V" said that the revolution had the sanction of the State Department and that the bankers made arrangements to produce the five million dollars for that purpose, but at the last moment, the higher officials of the State Department refused to sanction it." The state department was obviously afraid of the scandal an immediate dismissal of Canova might produce. He "resigned" from the state department on December 2, 1918, and the reasons were carefully concealed from public scrutiny. So well organized was this concealment that nearly fifty years later Louis M. Teitelbaum, a historian trying to find the reasons for Canova's resignation, wrote, "The cause of his leaving is nowhere stated in the published or private records available for study, a circumstance nearly unique in the State Department"; Teitelbaum, *Woodrow Wilson and the Mexican Revolution* (New York, 1967), 269. After his resignation, the state department carefully followed his every movement; *ibid.*, 411.

the obligations herein contained. (b) That the Mexican government will appoint your principals, with the character of financial adviser or fiscal agent, for the negotiation of all financial questions which are to be negotiated in the United States Such appointment as fiscal agent will also carry with it the designation of the bank recommended by your principals as depository of the Mexican government upon qualification as such in a proper way.

Point 10 (g) specified that a mission be nominated by the new government which would be empowered to negotiate with the United States concerning "the bases which are to serve for the following matters: Chamizal, the waters of the Colorado River, naval stations in the Pacific, the strategic military railroads of the Republic of Mexico; and to agree upon the appropriate measures that are to be taken so that your principals may supervise in Mexico the expenditure of the Funds secured by loans placed by them."

In Point 10 (i) it was stated that "To bring about the utmost harmony and cooperation between the government of Mexico and the United States . . . we are in favor of and will work to bring about the voluntary creation by the government of Mexico of military zones to cover and include the lines of all north and south trunk railways, now existing or to be hereafter constructed under a stipulation that will provide for a mutual offensive and defensive alliance and require both parties to protect the said zones in all cases of threatened danger." Point 10 (j) had as its purpose "the termination to a compromise satisfactory to them of the concession under which Sir Weetman Pearson, now Lord Cowdray, and his associates operate the railway line between Puerto Mexico and Salina Cruz, across the Isthmus of Tehuantepec, and the return of the said railway and the control and complete operation of the same to the government of Mexico." It did, moreover, grant to the parties involved, "the conditions being equal, the profits from the sale of the bonds to provide the necessary funds to enable the government of Mexico to bring about the termination of such concessions, to place the said railway in first-class operating condition, and to double-track the same for its entire length, with all the necessary equipment, terminals, etc." It was "understood," under the terms of the agreement, that "in all cases the Mexican government" would "retain control or majority of the stock of the railway company, granting to your principals, together with such compensation as may be agreed upon, such portion of said stock as may be proper but not exceed 49% of the same." Also included was a clause which provided that this railway would be made part of a military zone, which the United States would have the right to protect if it felt the railway was threatened.⁴⁹

Did a similar agreement underlie the 1915 Canova-Iturbide scheme? Although there is no conclusive evidence for such an accord, there is a strong probability that it existed. One indication is the remarkable similarity between the charges Villa leveled at the United States in his Naco Manifesto of 1915 and the provisions of the 1917 accord. U.S. interests were to have the deci-

⁴⁹ Bilingual unsigned agreement, November 1917, National Archives, Dept. of State, Leland Harrison File, box 208 (Mexican intrigue).

sive influence in selecting Mexico's secretary of foreign relations and secretary of finance. The United States was to grant a large loan to Mexico and supervise its finances in turn. The United States was to be given naval bases in the Pacific, mainly in Magdalena Bay. U.S. business interests were to share control of the Tehuantepec railroad, which hitherto had been under British control. Also included were other concessions to the United States that Villa had not mentioned in his accusations.⁵⁰

Since the same plotters, Canova and Iturbide, were involved in both schemes, there is good reason to assume that they were ready to enter into the same kind of agreement in 1915 that they did in 1917. And, although there is no definite evidence that the oil companies were as involved in the plot of 1915 as in that of 1917, that a man like Chandler Anderson, who was very sympathetic to oil interests, had backed Canova's plan in 1915 could indicate a similar support by oil companies. Certainly, there is no reason to assume that the oil interests had less compunction about supporting a plot in 1915 than in 1917. Carranza had forfeited their original support after he began raising taxes on oil.

IT IS NOW POSSIBLE TO GET A CLEAR IDEA of the events leading up to and motivating Villa's attack on Columbus. What may be termed Canova's "overt" plan of May 1915 would have sufficed to put an end to the Revolution and have transformed Mexico into a U.S. protectorate. If complemented by the "covert" plan of 1917, it would have spelled complete political and economic domination of Mexico: the overt provisions provided for an "unofficial administrative advisor" and U.S. supervision of customs; the covert provisions, for (1) U.S. intervention in Mexico whenever the United States perceived a threat to the operation of the railroads and to a number of important ports and other strategic facilities, (2) strong influence by U.S. business interests over the Tehuantepec railroad, and (3) probable control by the U.S. Navy over Magdalena Bay. Villa's army held a key position in the scheme. "I'm assured," Canova wrote Bryan, "that 20,000 men, mostly trained soldiers of the old Federal Army coming largely from Villa's ranks, would adhere to it; but, in all probability, Villa's entire army will join the movement." He also wrote that he had discussed those parts of the plan that he had revealed to Bryan with two representatives of Villa, of whom more will be said later.⁵¹

⁵⁰ Among the other concessions to its U.S. backers, the Mexican side was willing "to bring about the attachment and return to the true owners, by legal means, of the funds which the so-called Commission for the Regulation of Henequén has in this City . . ." and "(d) To bring it about that the Mexican Government grant to your principals, other conditions being equal, the preference for the upbuilding and operation in the Republic of Mexico, of the industries of iron and steel, and those related thereto, including the acquirement and exploitation of mines of iron. (e) That the Mexican Government will give facilities to your principals for the upbuilding of the sugar industry in the country, and will not impose new taxes or burdens which impede this industry. (f) That the Mexican Government will grant to your principals, other conditions being equal, the right to purchase the vessels to be sold because of having been interned by the Government by any reason." *Ibid.*

⁵¹ Canova to the Secretary of State, July 17, 1915, National Archives, Dept. of State Files RG 59, File 812.00 15331 1/2.

Villa did not for long remain in the dark about the contents of the plan, although it is not quite certain how he first found out. Perhaps, as he later reported, he was simply approached and offered U.S. support if he were to approve all the provisions contained in Canova's plan. (Canova may have felt that in May 1915 Villa had little to lose and that, having already been decisively defeated, Villa would be ready to settle for anything in order to retain at least some remnants of his power.) At any rate, it is certain that by the summer of 1915 news of the Canova-Iturbide plot had reached Villa's representatives in Washington, who were deeply incensed about it. William Teitelbaum, a journalist and businessman who kept in touch with the various Mexican factions represented in Washington, described in a letter to H. G. Wright, editor of the *New York Globe*, the reaction of Villa's emissaries to the plot:

On several occasions during the past weeks I have discussed with you the serious charges being made by Mr. E. C. Llorente, the accredited representative of the Mexican Convention forces, that Mr. Leon Canova, chief of the Mexican division of the State Dept., was secretly advancing the candidacy of Eduardo Iturbide for Provisional President of Mexico. . . .

A few days before General Angeles came to Washington, I called on Mr. Llorente, who complained with a degree of temper that Mr. Canova was complicating the Mexican trouble by injecting and fathering Iturbide's candidacy, which they would fight to the last ditch, even if backed by American forces. When, in proof of the statement all he could offer was that newspaper correspondents whose names he was not at liberty to divulge, kept him informed of the matter, I frankly told Mr. Llorente that it looked to me like a mote in his eyes. The same evening I carried the complaint to Mr. Canova for his guidance and he coolly dismissed the affair, saying, "I don't pay any attention to them. They are hard to please." which assured me that Mr. Llorente was mistaken.

A few days later I arranged an appointment to have Mr. Canova meet General Angeles, which was held in Mr. Canova's home. General Angeles, following the conference, claimed he felt so insulted by Mr. Canova's praise of Iturbide and the overtures he made in seeking General Angeles' cooperation, that a further appointment at the same place accepted by General Angeles before Mr. Canova "showed his hand" that General Angeles could not and did not meet.⁵²

Since Canova was the highest state department official directly concerned with Mexican affairs, there was every reason for Villa to presume that he acted on instructions from the administration. There was no way for Villa to learn that the cabinet had rejected Canova's plan. To the Mexican revolutionary, Canova's plan was nothing short of official U.S. policy.

When, in October 1915, shortly after inviting Villa's representatives to a peace conference in Washington, Woodrow Wilson reversed himself with inexplicable suddenness and recognized Carranza, even Roque González Garza, a far more moderate revolutionary than Villa, concluded that Car-

⁵² Wright judged the contents of the letter so significant that he transmitted it to Lansing. Teitelbaum to Wright, Aug. 17, 1915; enclosed in Wright to Lansing, Aug. 18, 1915, National Archives, Dept. of State Files, RG 59, File 812.00 15834.

ranza must have bought this recognition by agreeing to importunate U.S. demands. The following month Wilson went one step further and allowed Carranza's troops to cross U.S. territory in order to inflict a crushing defeat on the man he had backed only a short time before, and Villa rid himself of all lingering doubts as to the cause of the blatant reversal by the American president. He became firmly convinced that nothing less than Carranza's consent to the terms spelled out in Canova's original plan could have swayed Wilson so quickly. Little wonder, then, that Villa soon made his convictions known publicly and accused Carranza of having "sold out."

VILLA'S PUBLIC ACCUSATIONS FOUND NO RESPONSE. The Naco Manifesto fell on deaf ears. Neither Dieguez nor other Carranza generals (except one) deigned to reply to his letters. The people of Chihuahua did not flock to his banner after he exposed Carranza's plans from the balcony of the Municipal Palace. As a consequence of this poor response, Villa no doubt became extremely frustrated, his frustration further aggravated by Carranza's ability to assume an extremely nationalistic posture even after having consented—so Villa thought—to Mexico's conversion to a U.S. protectorate. For a time Villa hoped—as he told his collaborator Silvestre Terrazas—that Carranza would himself jeopardize his situation. If he applied any of the provisions of the pact, his generals would see Villa's accusations confirmed and turn against their chief. If he did not comply with the provisions, Wilson would drop him and Villa's own movement might revive.

As the weeks passed and none of these hopes was realized, Villa's anger and despair mounted. It is a measure of the depth of that despair that Villa seriously proposed to Zapata that he abandon his home territory and cross wide sections of Carranza-controlled land to attack the United States. Villa's main problem was that he had no way of substantiating his charges—with one exception: his accusation that Carranza had given U.S. troops the right to enter Mexican territory in return for permission to cross the United States to rescue Agua Prieta from Villa's attack. In his letter to Flores and Dieguez, Villa had already touched on the issue. "If American troops enter Mexico," he asked, "what will you do?" This issue remained uppermost in his mind for a long time. In the first manifesto he issued after Pershing's invasion, he railed against Carranza:

With the greatest good faith, I have remained inactive with my forces in the hope that the activities of the so-called Constitutionalist Government would be directed toward repelling the invasion and securing the re-unification of the Mexican people. But . . . far from trying to expel the invader, this government utilized with the greatest perversity the gravity of international relations for personal gain and without in any way respecting the honor of Mexico.⁵³

⁵³ For a full text of this manifesto, see the appendix to Antonio M. Delgado, ed., *Romance histórico villista* (Chihuahua, 1975), 172–73.

By attacking the United States and inviting possible reprisals, Villa hoped to create an insoluble dilemma for Carranza. If the latter allowed U.S. troops to penetrate into Mexico without offering resistance, Villa hoped to expose Carranza for what he thought he was: a tool of the Americans. If Carranza refused to have himself exposed and simply ignored the original agreement and resisted the Americans, so much the better. The tie between him and the Wilson administration would have been ruptured and his position severely shaken. Max Weber, the German vice-consul in Ciudad Juarez, wrote to a business partner in the United States in December 1916, "Villa wants intervention and stated in public in Chihuahua that, as long as the washwoman in Washington is at the head, he will continue to burn and loot until America intervenes in Mexico and brings about the downfall of Carranza."⁵⁴

One more question needs to be considered: What role, if any, did Germany's much-rumored involvement play in Villa's decision to attack Columbus? In May 1915, Bernhard Dernburg, Germany's propaganda chief in the United States, without consulting Count Bernstorff, the German ambassador, submitted a plan to Admiral Henning von Holtzendorff (a high official in the German admiralty who was soon to become its head) to use Villa in order to provoke U.S. intervention in Mexico. Dernburg reported that Felix Sommerfeld, Villa's representative in the United States, had told him that he (Sommerfeld), while engaged in negotiations two months earlier between Villa and the U.S. chief of staff at the Arizona border, could easily have provoked U.S. intervention. But Sommerfeld stated that he had not done so because he was not sure whether German authorities wanted such an intervention. He did point out that a U.S. invasion of Mexico would stop U.S. arms shipments to the Allies and distract U.S. attention from the European theater. He also told Dernburg that he "was convinced that an American intervention in Mexico could be brought about."⁵⁵

Holtzendorff judged the matter to be of such gravity that he submitted it for approval to Germany's secretary of state, Gottlieb von Jagow, who gave it his wholehearted approval. He wrote,

In my opinion, we must answer "yes." Even though I am not fully convinced that deliveries of munitions can be stopped entirely, it would be very desirable that America be drawn into a war and be distracted from Europe, where it tends to be pro-English. It will not intervene in Chinese affairs, so that an intervention in Mexico would constitute the only distraction for the American government. Since we cannot now do anything in Mexico, American intervention would also be best for our interests there.⁵⁶

Sommerfeld obviously had undertaken extensive preparations for carrying out this plan. In the same month that Jagow gave his approval, two men—J. M. Keedy and Eduardo Linns—turned up in Washington as representatives of

⁵⁴ Max Weber to Major Britton Davis, Dec. 5, 1916, Private archive of Max Weber, El Paso, Texas.

⁵⁵ Dernburg to Holtzendorff, May 1915, German Foreign Ministry Archives, Bonn, Mexico 1, Secret, vol. 1.

⁵⁶ Jagow to Holtzendorff, May 1915, *ibid.*

Pancho Villa. They had never before and were never again linked with Villa in any way. According to the project for counterrevolution in Mexico that he submitted to Bryan, Canova negotiated primarily with these men to win the support of the Villista faction for his plan. Actually, the two men were German agents whom Sommerfeld had probably recommended to Villa as skilled negotiators, capable of wielding influence in the United States. In several reports by Kurt Jahnke, who headed German naval intelligence in North America from 1917 to 1919, Keedy is mentioned as Jahnke's collaborator.⁵⁷ Linns was suspected in 1917 by U.S. authorities of being linked to Germany.⁵⁸

There is no information about what role these men played in the negotiations with Villa. They may very well have revealed to him those aspects of the Canova-Iturbide conspiracy that Canova would have kept secret. They may also have suggested to Villa that Carranza was involved in the negotiations with Canova. The one thing they did not need to do was to invent a plot that would have imposed a quasi-American protectorate over Mexico. Canova, Iturbide, and their allies among Mexican conservatives and U.S. business interests had done that already.

Nor did the German government itself believe that any of its agents had caused or were even involved in the raid on Columbus. On March 28, 1916, a few weeks after the attack, Bernstorff, who had not been informed of Sommerfeld's plan, wrote to German Chancellor Bethmann-Hollweg, "It is not surprising that the attempt was made to state that German intrigues were responsible for Villa's attack and to depict Germany as the disturber of the peace. Naturally, no proof of this erroneous statement was forthcoming." An official of the German foreign office, probably Max von Montgelas, the head of the Mexican desk, added the word "*leider*" ("unfortunately") before the word "erroneous." He was regretful that, as far as he knew, Germany had not been involved in the Columbus attack.⁵⁹

⁵⁷ Keedy was first referred to as a German agent by Customs Agent Zachary Cobb; see Cobb to the Secretary of State, Oct. 2, 1917, Dept. of State Files RG 59, File 862.202 12/742, MC 336 roll 59. In an unsigned report to Wolfgang Kapp, Jahnke describes extensive negotiations with Keedy; Kapp papers, Deutsches Zentralarchiv Merseburg, Rep. 92, El. no. 13.

⁵⁸ Linns was placed on a confidential "Trading with the Enemy" list. U.S. Ambassador Fletcher, in a report to Lansing on July 27, 1918, called him "very pro-German." Teitelbaum, *Woodrow Wilson and the Mexican Revolution*, 419.

⁵⁹ This evidence does not entirely rule out the possibility of German sponsorship of or participation in the attack. There are some indications that Sommerfeld remained in touch with Villa both before and after the Columbus raid. In March 1917, Mexican authorities suspected him of supplying arms to Villa; see Monteverde to the Mexican Consul in Los Angeles, March 7, 1917, Archivo de Relaciones Exteriores, Mexico. On the basis of interviews with one of Villa's former lieutenants, James Sandos raises the possibility that Dr. Rauschbaum, who was of German origin and served as Villa's personal physician and financial adviser, may have induced Villa to attack Columbus by reporting that a bank in that town (the Columbus State Bank with which Villa had dealings) had cheated him. Sandos, "German Involvement in Northern Mexico, 1915-1916," 70-89. On the whole, this evidence is not sufficient for assuming German responsibility for the raid. The best sources for a definitive answer to this question would be the files of the German intelligence agencies. Whatever files of German Army Intelligence (about whose involvement in the Villa intrigue there is no evidence) that survived World War I were destroyed by bombs in World War II, together with German military archives in Potsdam. The records of the German navy, whose clandestine services were those principally involved with Villa, survived World War II and are now available to researchers at the Deutsches Militärarchiv in Freiburg im Breisgau. During a recent visit to this archive, I

IN THE SHORT RUN THE ATTACK PROVED BENEFICIAL to Villa, in almost the same way he thought it would. For many months—as long as Carranza seemed incapable of expelling Pershing's punitive expedition—Villa became a symbol of national resistance in Chihuahua. Battered and nearly destroyed in December 1915, his army multiplied, reaching several thousand by September 1916. It even retook the capital city of Chihuahua and held it against Carranza's forces. Its strength ebbed again only after the punitive expedition left Mexico in February 1917. A further benefit of the raid was Villa's receipt of several coffins filled with arms shipped from Germany after the attack. They had apparently been purchased from an arms factory in Bridgeport, Connecticut before the outbreak of the war.⁶⁰

Carranza, too, was deeply affected by Villa's attack, though not in the fatal way Villa had hoped for. Mexican-American relations deteriorated and stopped just short of open belligerency. From March 1916 onward, Wilson imposed a weapons embargo on Mexico—never strictly observed—which was to last until the fall of Carranza in 1920. The latter, deprived of easy access to arms and despairing of his ability to expel the punitive expedition, successfully sought a rapprochement with Germany, which continued even after the exit of Pershing's troops from Mexico. Villa, meanwhile, faced a severely shaken Carranza and survived five hard and savage years of guerrilla warfare until Carranza was overthrown in 1920 and Villa made peace with his successors.

But what of Villa's avowed main purpose in attacking the United States—

was informed that the most sensitive files relating to sabotage and secret operations during World War I were destroyed by the navy itself after the war. The only reports on the clandestine activities of the intelligence service of the German navy that I managed to locate are contained in the German Foreign Office archives in Bonn and among the papers of a leading German politician, Wolfgang Kapp, now in the German Democratic Republic: Kapp Papers, Deutsches Zentralarchiv Merseburg, Rep. 92, El. no. 13. There I found an anonymous report on the activities of German naval intelligence in Mexico between 1917 and 1919, which was probably written by Kurt Jahnke, who headed German naval intelligence in North America from 1917 to 1919 and who had already been a German agent in 1916. Neither in these reports nor in the telegrams of the German secret service intercepted by the British is there any indication of German sponsorship of Villa's Columbus attack. While rejoicing at Villa's attack, the Austrian Foreign Office (which carefully monitored the activities of its closest ally) never attributed it to Germany. Austrian Ambassador in Washington to the Minister of Foreign Affairs, April 17, 1916, Haus-, Hof-, und Staatsarchiv Wien, PA Berichte Mexico. A more important indication than all of this is that the German navy, had it been responsible for Villa's attack, would have had every reason to claim the credit with other branches of the German government. In 1916 the navy was engaged in a growing power struggle with other government agencies because of its demand for unrestricted submarine warfare. A successful operation such as the organization of Villa's attack on the United States, a plan which had previously obtained the wholehearted endorsement of the German Foreign Office, would certainly have enhanced the navy's prestige with the rulers of Germany. I have found no evidence for any claim of this kind by the navy in any of the relevant archives. Neither the files of the German Foreign Office in Bonn, Potsdam, and Merseburg nor the papers of the Reichskanzlei in the Deutsches Zentralarchiv in Potsdam contain any evidence for Germany's involvement in the Columbus raid. The same is true for the diaries of Kurt Rietzler, a close collaborator of the German chancellor, which contain some of the most sensitive data that reached Chancellor Bethmann-Hollweg during the course of the war. Kurt Rietzler, *Tagebücher, Aufsätze, Dokumente* (Göttingen, 1972). All that one can say is that, immediately after receiving news of the raid, the German Foreign Office sought ways to supply Villa with arms; the head of the Mexican desk at the German Foreign Office wrote a memorandum to this effect in March 1916. Memorandum by Montgelas, March 23, 1916, German Foreign Office, Bonn, Mexico 1, vol. 56.

⁶⁰ Emmanuel Voska and Will Irwin, *Spy and Counterspy* (New York, 1940), 167.

the preservation of Mexico's independence? Villa actually increased immeasurably the real threat to his country's independence. U.S. troops penetrated into Mexico, and, in 1916, the United States sought to impose conditions severely limiting Mexico's sovereignty as a prerequisite for the withdrawal of its troops. And if Villa, by attacking Columbus, had sought to alert Mexican and U.S. public opinion to the threat to Mexico's independence that loomed uppermost in his mind—plots by U.S. interests to convert Mexico into a protectorate—he failed completely. Subsequent events not only failed to bear out the charges of his Naco Manifesto insofar as Wilson and Carranza were concerned, but they even failed to direct attention to the very real foundations that underlay some of those charges. It was Villa's misfortune to have been right in his general suspicions, but wrong in his specific assumptions. Plots like the one he outlined at Naco, that is, involving the same purpose but different names, continued to be hatched and to escape public notice. Only a few months after Secretary of State Lansing discovered and rejected Canova's plot to secure domination over Mexico for Standard Oil and other U.S. business interests in December 1917, a new scheme had already been conceived.

For this pact the politician involved was Alfredo Robles Dominguez, a long-time adherent of Carranza, who was now willing to aid in the overthrow of his erstwhile leader. In a conversation between a representative of Robles Dominguez and a British diplomat in Washington, the Mexican politician let it be known that his movement was to be financed by "the International Harvester Company, the St. Louis Car Company, certain oil interests represented by a Mr. Helm, and others."⁶¹ A few days later, the British representative in Mexico, Cunard Cummins, reported on some of the conditions Robles Dominguez had accepted as a price for gaining the support of these U.S. companies, and it is worthwhile to quote them for the similarity they bear to previous agreements. Robles Dominguez agreed

(a) To act in Mexico and Latin America as the interpreter of President Wilson's policy of confidence and fraternity between the United States and Latin America.

(b) To establish here the "Bank of Mexico," the Consultative Board of which composed of British, Americans, French, and Mexicans, two of each, will handle and check the receipts and expenditure of the Government. It will practically be the Ministry of Finance though for appearances sake a Mexican Minister, an obedient dummy, will be appointed.

(c) To deliver to the League of Nations Magdalena Bay which on account of the interest displayed in it by Japan and the United States threatens to involve Mexico with both. He presumes that the League would place this strategic point in the hands of the United States for use as a naval base on the Pacific and that through the intermediation of the League, Mexican interests would be duly protected.

(d) To bring up for prompt settlement all pending questions with the United States which serve as a cause of friction. The "Chamizal" dispute, the difficulties of the Tlahualilo Company, etc., etc., he would settle in a spirit favourably disposed towards the foreign interests.⁶²

⁶¹ Hohler to Foreign Office, April 11, 1918, Public Record Office, London, Foreign Office 371 3244 2658.

⁶² Cummins to Foreign Office, April 1918, *ibid*.

The failure to implement these plans was due in the main to the unwillingness of both the U.S. government and some important U.S. business interests to carry out any aggressive policy in Mexico as long as the United States was involved in World War I. It was also due, however, to Wilson's objection in principle to establishing a U.S. protectorate over Mexico.

If Villa's attack on Columbus had done much to imperil Mexico's precarious independence, the failure of the Pershing expedition did much to repair the damage. Ultimately, it convinced the American public, as well as the U.S. military, that future intervention in Mexico would be more difficult and costly than had been assumed. In 1914 Secretary of War Lindley M. Garrison estimated that fourteen U.S. divisions would be needed to occupy Mexico;⁶³ by 1918 that estimate had to be revised upward. In April of that year the British general staff reported that the U.S. military now believed that, at the very least, twenty divisions (five hundred thousand men) would be needed.⁶⁴ The failure of the Pershing expedition, however, not only enhanced Mexico's standing vis-à-vis the United States, but also Villa's standing with many of his own countrymen. In the pantheon of popular legend, expressed in countless stories and *corridos*,⁶⁵ he is still celebrated as the man who attacked the United States—and got away with it.

⁶³ Richard D. Challener, *Admirals, Generals, and American Foreign Policy, 1898–1914* (Princeton, 1973), 351–52.

⁶⁴ Memorandum of the General Staff, May 9, 1918, Public Record Office, London, Foreign Office 371 3244 2658.

⁶⁵ The legends about the attack on Columbus have been compounded by the fact that Villa never officially assumed responsibility nor gave an explanation for the attack. As a result, some authors have gone as far as to claim that the raid was a provocation by interventionist groups in the United States. See, for example, Edgcomb Pinchon, *Viva Villa!* (New York, 1933), 338–39. The facts I have established here as well as the testimony of a large number of witnesses and participants leave no doubt about Villa's involvement. See Clendenen, *The United States and Pancho Villa*, 234–47; Calzadiaz Barrera, *Porque Villa atacó Columbus*; and the testimony of Juan Caballero. In the manifesto he issued after Pershing's invasion, Villa neither claimed nor disclaimed responsibility. For the text of the manifesto, see Delgado, *Romance histórico villista*, app., 172–73. One of the reasons for his attitude may have been that he began to doubt the veracity of the secret Carranza-Wilson pact. It is significant that, although in the manifesto Villa accused Carranza of capitulating to the Punitive Expedition, nowhere in the manifesto nor at any time thereafter did he again refer to the secret pact, which had figured so prominently in his Naco declaration. By 1917, Villa may have had additional reasons for avoiding any responsibility for the Columbus raid. The adoption of the Mexican Constitution of 1917, as well as Carranza's German-inclined neutrality, led to a worsening of relations between the Carranza administration and the U.S. government. Under the circumstances, Villa may have hoped to obtain U.S. neutrality, if not support, in his fight against Carranza.

Reviews of Books

GENERAL

FRANCO CATALANO. *Metodologia e insegnamento della storia*. (Collana di aggiornamento e didassi, number 27.) Milan: Feltrinelli. 1976. Pp. 277. L. 4,300.

The reader who is not familiar with the work of Franco Catalano may gather from the title that this is a technical work on the use of historical evidence and on the instruction of history. Perhaps, but it is chiefly a spirited presentation of a Marxist view of history by a noted Italian historian. Catalano has emended the eleventh thesis on Feuerbach. Where Marx argued that "philosophers have only interpreted the world in various ways when the point is to change it," Catalano wants historians to do the philosophers' job. If they do it in a suitably radical way, Catalano thinks, they will help precipitate proletarian revolution.

The first step for Catalano is to reform the historians of Italy. He wants them to drop their studies of the Renaissance and the Risorgimento in favor of new topics. He urges them to catch up with their colleagues in other countries who have long since learned to make use of such ancillary disciplines as demography, climatology, and geology. Catalano asks, in brief, for an *aggiornamento* of Italian historical scholarship. There are, however, limits to Catalano's desire for innovation. He has little patience with sociology (because it treats society as unchangeable) and none whatsoever with structuralism (because it considers man as immutable).

Catalano considers Italian historical scholarship to be backward, and he assigns the blame for this to Benedetto Croce. This may be giving Croce more credit or discredit than he deserves. The discussion of Croce is polemical: he is presented as a temporizing idealist and as a bourgeois fossil. This sort of invective, characteristic though it may have been of Marxist writing during Croce's lifetime, now seems gratuitous. Perhaps Catalano has still not recovered from his own youthful disillusionment with Croce. Most disturbing of all is

his contention that Croce's thought was determined by the bourgeois society from which he sprang, since this assertion denies the autonomy of thought which Catalano elsewhere seeks to uphold.

These points aside, this book has worth as a piece of primary evidence, for as a whole it testifies to the troubled state of contemporary Italy. Here the reader is presented with a noted scholar who awaits the revolutionary millenium and who thinks that that millenium would have come in 1968 if only the PCI had won over the peasantry. This is a book that teaches us as much about revolutionary expectation as about historiography.

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THOMAS J. SCHLERETH. *The Cosmopolitan Ideal in Enlightenment Thought: Its Form and Function in the Ideas of Franklin, Hume, and Voltaire, 1694-1790*. Notre Dame: University of Notre Dame Press. 1977. Pp. xxv, 230. \$13.95.

Although France was indubitably the spiritual home of the Enlightenment, furnishing its inspiration and its militancy, the movement's centripetal pull toward Paris was offset by the centrifugal force of its cosmopolitanism, giving it variety through its multinational connections and breadth through the conscious antiparochialism of its convictions. Progress, to the mind of the Enlightenment, depended upon breaking out of tribal and sectarian limitations to the realization of the unity of mankind and the universality of truth.

Thomas Schlereth has set out to anatomize the significant theme of cosmopolitanism in the Enlightenment, to explore the convergence of intellectual forces that generated it, and to trace out its implications in theory and practice in the world of the philosophes. His definition of the concept is a broad one. He sees it as an ideal which aimed at the transcendence of national and parochial prejudices, at the cultivation of urbanity and well-

travelled ease in social style, and at the development of loyalty to the whole of the human race. Although it was not a fully realized or even fully comprehended ideal—one sometimes feels that the philosophes' cosmopolitanism was really nothing but an ethnocentric projection of their own self-image on the world—this does not lessen its importance as an informing idea in their writings.

Schlereth's strategy is to divide the central theme into categories and to illustrate each category with a multitude of quotations, supplied principally by Hume, Voltaire, and Franklin, but also supported extensively from their fellow philosophers of all nations. In this way he deals with the sociology of the Enlightenment as an international movement, the universalist implications of science, the eclecticism of the Enlightenment's philosophical and moral concerns, the syncretistic character of its religious concerns, and the antinationalism of its political and economic doctrines. The work concludes with a brief discussion of the displacement of cosmopolitanism by post-Revolutionary nationalist fervor. There is also a useful bibliographical essay.

As Schlereth acknowledges, he owes much to the work of Peter Gay. Indeed, he owes somewhat more than he acknowledges. This reviewer detected two unattributed verbatim borrowings (on pages 15 and 126) from Gay's *The Enlightenment: An Interpretation* (volume 1, pages 16 and x, respectively). In addition, it must be said that the publisher has not served the author well. Better copy-editing would have rescued him from occasional stylistic lapses and better proofreading would have saved the book from errors too numerous to list.

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I. I. ROSTUNOV, editor. *Istoriia pervoi mirovoi voiny, 1914–1918* [A History of the First World War, 1914–18]. In two volumes. Moscow: Izdatel'stvo "Nauka." 1975. Pp. 445; 606. 2 r. 19 k.; 3 r. 04 k.

In his capacity as Chief of the Institute of Military History under the USSR Ministry of Defense, Lieutenant General P. A. Zhilin presides over an impressive research and publication effort within the general area of Soviet military historical studies. The importance of General Zhilin's Institute derives in part from the overall significance ascribed to history by Marxist-Leninist theory and in even larger part from the didactic and patriotic functions attributed to military history by the present Kremlin leadership. It is no accident that the media of the USSR constantly hold up for public view the military experiences of the Soviet Union.

I. I. Rostunov's two-volume edited study is intended to fill what General Zhilin's Institute con-

siders an important gap in Soviet military historical research. The purpose of these two volumes is to make available to the educated Soviet reader a comprehensive survey history of World War I, the first such study on this scale since the 1920s, when early Soviet historians eagerly scrutinized the Great War for any lessons it might hold for the military forces of the fledgling Soviet state. Presumably these volumes will also serve an instructional function within institutions of both military and civilian higher education. They possess all the strengths and weaknesses logically proceeding from the current Soviet approach to military history.

On the plus side, these volumes present a coherent, readable survey of World War I from what the editor calls the "position of Marxist-Leninist methodology." Rostunov and his collective of seven authors, sometimes separately and sometimes jointly, offer eight distinct chapters narrating the causes of the war and its course on the various fronts. The first three chapters (volume 1) constitute a lengthy introduction in which the authors painstakingly set the stage for the conflict, including an examination of the origins of the war, the armed forces of the participants, and the military plans of the major powers on the eve of hostilities. The remaining five chapters devote attention successively to each succeeding year's campaign in a conflagration which produced some of the most momentous changes of the twentieth century. Throughout the narrative, the authors consistently attempt to base their account on sources which would be considered novel to the Soviet public.

On the negative side, these volumes suffer from the usual shortcomings, chiefly philosophical in nature, stemming from an overly narrow, didactic approach to history. First, there is the temptation to see too many lessons in the past. Then, there are other difficulties associated with a historical view which conceives of the past as a kind of morality play in which identifiable good triumphs over identifiable evil. For example, an examination of the origins and consequences of World War I emerges largely as rewarmed Hobson and Lenin with occasional and highly selective dashes of Fritz Fischer.

Despite these shortcomings, the impressive resources allocated to this study suggest that it will likely remain for some time the standard Soviet account. For vision and originality, it still falls short of A. M. Zaionchkovskii's pioneering studies.

BRUCE W. MENNING

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A. A. GRECHKO et al. *Istoriia vtoroi mirovoi voiny, 1939–1945* [History of the Second World War, 1939–45].

In twelve volumes. Moscow: Izdatel'stvo "Voennoe Izdatel'stvo Ministerstva Oborony SSSR." Volume 7. 1976. Pp. 551. 2 r. 80 k.

The seventh volume of what the Soviets call the "fundamental scientific" history of World War II covers the events from April until the end of 1943. For the Soviets, the period was marked by a military success—the victory at the Kursk salient—and a military/political disappointment—the failure of the Anglo-American Allies to open a second front in Europe in 1943.

The second-front delay is considered by James MacGregor Burns (whose *Roosevelt: The Soldier of Freedom* [1970] is cited in this volume) as the chief factor contributing to Soviet anger and cynicism over the Allied war effort. The present volume contains ample evidence that this cynicism persists. Throughout, British and American military policy is depicted as one of limiting the commitment of their own forces while allowing the Soviets and Germans to exhaust theirs in combat.

The Soviet victory in the battle of the Kursk salient is presented as not only a triumph of Soviet military art but also a victory of the Soviet war economy over that of a Germany in control of almost all the resources of Western Europe. The Soviet decision to accept the German attack on the salient was based, in part, on Soviet confidence that their arms production exceeded that of the Germans and that the Soviet forces could replace equipment losses faster than the Germans could.

Operation "Citadel," as the Germans called it, had as its objective the envelopment of the million or so Soviet troops in the salient. Originally planned to start in early May when the spring thaw was over, "Citadel" did not start until July 5th. The delay was caused by Hitler's uncertainties and his desire to wait for new tanks.

The German attacks achieved their greatest success on the southern shoulder of the salient. Here troops of Von Manstein's Southern Army Group by July 12th penetrated the Soviet positions some forty kilometers. A Soviet counterattack led to what the Soviets claim was the largest tank engagement of the war, involving a total of twelve hundred tanks and self-propelled guns. The Soviet victory in this battle marked the collapse of the German thrust from the south (p. 154).

Von Manstein, however, claims in *Lost Victories* (1958) that on July 13th, with the battle at its climax, Hitler summoned him and the Central Army Group commander (Von Kluge) and announced that because of the Allied landing in Sicily and the situation in Italy, "Citadel" would have to be discontinued. This episode is not mentioned in the Soviet description of the battle. In fact, an attempt is made to prove exactly the oppo-

site with a quote from the German war archives that the Allied landing in Sicily on July 10th did not influence the German plans for Operation "Citadel" (p. 153). By not mentioning Hitler's meeting on July 13th, the Soviet version gives the reader without access to outside sources further proof that the Anglo-American efforts were indeed inconsequential.

WILLIAM J. SPAHR
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AMOS PERLMUTTER. *The Military and Politics in Modern Times: On Professionals, Praetorians, and Revolutionary Soldiers*. New Haven: Yale University Press. 1977. Pp. xix, 335. \$15.00.

Amos Perlmutter distinguishes three military types in modern times: the professional, the praetorian, and the "noncorporate revolutionary soldier." It is worth quoting his description of the third type to illustrate his use of categories as well as their possibly excessive fluidity. Noncorporate revolutionary soldiers, he writes, "have ranged from anticorporate romantic revolutionaries at one end of the continuum, through national liberation fighters (Marxist and non-Marxist), to routinized revolutionary professionals. Although in the 1970s the revolutionary soldier is essentially noncorporate, he does exhibit some corporate characteristics. The institutionalization of the revolutionary professional in China and Israel may orient him more firmly toward corporatism, though it is unlikely that he will ever become 'corporate' in the nineteenth-century sense or, for that matter, in the sense of twentieth-century praetorianism" (p. xv). Further, he argues that corporatism develops from professionalism, and that corporatist attitudes have triggered political intervention by the military throughout the world.

As can be seen, the book is part of a debate among political scientists, who as a group are more receptive to abstractions than historians tend to be. Nevertheless, historians will welcome any serious effort at clarifying the nature of civil-military relations in the recent past, and have themselves contributed to its exploration. The book raises a second issue of interest to historians, the author's attempt to integrate conceptual analysis with comparative history. It must be said that his use of the latter is not a success. Errors abound, ranging from wrong names, dates, and titles—"the German defense ministry under Field Marshal Erich Ludendorff" (p. 11)—to major contradictions and misconceptions.

Two sentences will indicate the level of historical interpretation. A section on France asserts: "That the collapse of legitimate authority after 1789 did

not give rise to a praetorian civil-military relationship must be attributed to the strength of the army's corporate tradition" (p. 60). We assume that the Napoleonic experience does not fit the work's definition of praetorianism until we read: "A modern praetorian state is one in which the military tends to intervene in the government and has the potential to dominate the executive" (p. 93). A discussion of Prussian reforms singles out "the creation of the Allgemeine Kriegsdepart[e]ment, a military cabinet run by an Oberkriegskollegium, a small group of officers serving as personal aides to the king" (p. 50). Far from consisting of royal adjutants, the Oberkriegskollegium opposed their influence. It disappeared during the war of 1806 and some years later was succeeded by the Allgemeine Kriegsdepartement, which was neither a military cabinet nor a ministry of war, as Perlmutter's next sentence calls it, but jointly with agencies he does not mention formed such a ministry.

The author enters the thicket of institutional history to illustrate the development toward the "integrity of military professionalism" and the unity of command. The reality was more complex. The reformers established the Allgemeine Kriegsdepartement to assure unity of command and rationalize administration, but also to deprive the Crown of its former absolute authority—in short, to create a new division of power based in part on the recognition of professional expertise, in part on a new conception of the state. It is almost a truism that in the formative period of nation states everywhere, professional soldiers engaged in politics. At times professionalism was itself a highly political stance. One reason why the author's historical case studies are inadequate is that he asks the wrong questions; another is that his theoretical arguments do not derive from a continuum of past and present but look to history for illustrations and confirmation.

Whether this reduces the value of Perlmutter's arguments for political scientists I am unable to judge. Much that strikes me as laboriously labelled or self-evident in his theories may to others seem stimulating and constructive. But I suspect that the book's strength lies less in history or theory than in its many pragmatic observations on contemporary conditions, which offer correctives to widely-held assumptions about the role of soldiers in developing societies.

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KARL KORSCH. *Karl Korsch: Revolutionary Theory*. Edited by DOUGLAS KELLNER. Austin: University of Texas Press. 1977. Pp. vi, 299. \$14.95.

This volume makes available in English a sampling of the writings of the revolutionary theorist and activist, Karl Korsch (1889–1961). The recent interest in Korsch is part of a larger concern to rediscover an "original" Marxist theory, one free of both the determinist accretions of the social democrats and of the philosophically primitive, politically repressive heritage of the Stalinists. Numerous articles on Korsch have appeared in Europe and the United States in the 1960s and 1970s, including the entire winter issue, 1975–76, of *Telos*. A complete edition of his works is being prepared in Germany, and a full-length study of his thought was published in 1976 (Michael Buckmiller, *Karl Korsch und das Problem der materialistischen Dialektik*).

Well over half of this collection is taken up with introductory and explanatory text, which is welcome because Korsch's writings are not always models of precision and because both his political and theoretical positions altered in sometimes startling ways over the years, to the confusion of many observers, even those closest to him. However, the editorial zeal of Douglas Kellner is at times overdone: material in the initial general introduction is often repeated in the shorter essays that precede each of the selections from Korsch, and rarely do Korsch's own words say substantially more than what has been so amply laid out in the introductions. For the most part Kellner writes with insight, clarity, and intelligence, although some readers will wince at his frequent recourse to such terms as "fascistization," "scientization," and "concretized."

While briefly noting some of the weaknesses and apparent contradictions of Korsch's theory and action, Kellner appears to be far more impressed with Korsch's "brilliant critiques" and "penetrating insights," with his position as "one of the most interesting, neglected, and relevant theorists of the century." From the evidence presented in this volume these claims appear excessive. Korsch's theoretical and, above all, practical accomplishments (in his mind the two were strongly interdependent) were of an extremely uneven quality. His writings on revolutionary councils and socialization immediately following World War I merit respectful reading, but his pronouncements while a leading figure in the Comintern are scarcely less tiresome or free of cant than those of the typical Stalinist sycophant. Even the immediate postwar writings, as Kellner recognizes, are flawed by a lack of any coherent statement concerning how Korsch's ideal of nonbureaucratic socialization and decentralized workers' control would be put into effect. This seemingly incomplete and often abstruse quality characterizes much of Korsch's thought. What he actually meant by such frequently repeated phrases as "the

identity of objectifying knowledge and activity” remains elusive, one is tempted to say, mystical. And his political judgment—integral to his theory—was repeatedly defective and utterly lacking in realism. His description, for example, of all Communist, socialist, and bourgeois parties (in the Soviet Union, in Blum’s France, Chamberlain’s England, and FDR’s United States) as fascist in essence, devoid of progressive qualities, and thus worthless to the international working class as part of a Popular Front in combating fascism in Germany, Italy, and Spain, strikes this reviewer as anything but brilliant, penetrating, or relevant.

Because of the breadth, richness, and nuanced complexity of much of Korsch’s thought, it is nearly impossible in a short review to provide a fair treatment of it. Thus the above critical remarks may provide an overly negative impression both of Korsch’s thought and of Kellner’s treatment of it. That would be unfortunate because this is a scholarly and timely volume that many readers will no doubt find extremely useful.

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ANCIENT

KURT BITTEL. *Die Hethiter: Die Kunst Anatoliens vom Ende des 3. bis zum Anfang des 1. Jahrtausends vor Christus*. (Universum der Kunst, number 24.) Munich: Verlag C. H. Beck, 1976. Pp. 360. DM 148.

This book contains the original German text and documentation of a volume also published in French as part of the series *L’univers des formes* initiated by André Malraux and André Parrot. Kurt Bittel, the leading scholar in the field of Hittite archeology, and excavator of the Hittite capital from 1931, here presents a general view of the Hittites and their art in the perspective of long experience and new developments. The historical concept of the Hittites came into focus after the discovery of the archives at Boğazköy-Hattusha in 1906–07 (as is sketched in the introductory chapter), and a reasonably correct historical image existed by 1939. With the excavations of the 1950s and 60s, however, along with the material evidence for art and architecture, more tablet archives (Hattusha, Kültepe-Kanesh, Mashat, Ugarit) and remarkable archives of sealings on bullae (Karahüyük near Konya, Acemhöyük) have come to light, and new analyses have been made of texts and monuments. Hittite art and architecture are presented here in their newly revised historical context.

A chapter on the pre-Hittite era examines the

find complexes of the “royal” graves at Alaca Hüyük and Horoztepe with their metal sculptures of ritual character; the date given here is rather late, 2200–1900 B.C., in conjunction with the interpretation of the dynasty of Troy II (pp. 47–48). One cannot lightly reject these suggestions, the grounds for which are clearly explained, but controversy will continue, as it will also in relation to the Aegean chronology (cf. the chronological chart in the back of the volume).

The Anatolian world of the 19th–18th centuries B.C., the first chapter of the main Hittite section, is presented here in its astonishing wealth of architecture, art, and iconography. New material such as seal impressions and ivories from the large city at Acemhöyük is given prominence. Bittel points to contacts not only with Old Assyrian and Syrian traders, but also with Egyptian and Aegean cultures. Anatolian art of this period inherits basic features of “Hattian” art of the third millennium B.C.; religion and cult continue to be the motivation of most art.

The Old Kingdom and the Empire (1600–1200 B.C.) in modern interpretation show a gradual development as important works of major and minor arts are now understood to belong to early stages of the Hittite evolution. Notable are the discussions of the Bitik vase and related relief vessels with ritual scenes, now dated ca. 1600 B.C.; of early figurines and relief fragments from the fifteenth century; of the important silver rhyton and other cult paraphernalia in the Schimmel collection, dated fifteenth to fourteenth century; and of the Alaca Hüyük reliefs, attributed to the fourteenth century B.C. Now we begin to gauge the continuity of pre-Hittite, Old Hittite, and Middle Hittite art, and can reconstruct the cultural and religious background of the kings who are emerging in clearer order from the analysis of the archives and paleography.

Special attention is given the rock reliefs and the open air sanctuary at Yazılıkaya, the latter analyzed in the framework of Hurrian affinities of the art and dynasty of the Hittite Empire. The Şarıkışla axe illustrates some of the challenging perplexities of the era. Among the monumental discoveries is the building to the Southwest of Temple I in Hattusha, a complex identified tentatively as “é-giš-kinti,” *Haus der Arbeitsleistung*, where priests and scribes worked and lived.

The Anatolian Iron Age is dealt with in terms of Hittite survivals in the art and traditions of the new principalities of East and Southeast Anatolia, thriving in some places, blending with Phrygian, Syrian, Aramaic, Assyrian, and Phoenician elements elsewhere, and exercising an influence of its own on the Greeks as they entered more deeply inland.

The illustrations are splendid as well as scholarly in their careful selection and juxtaposition. The color plates are striking; even when a closeup of a detail is given for effect, a full view is given elsewhere. Good plans and maps are provided, as are chronological charts, a bibliography, a glossary, and a helpful list of data for each illustration (the list is better than in the French edition).

The text has to be read carefully. It is a wise, but also probing statement about an ancient land and culture which Bittel has come to know and understand better than any of us in scholarly and human depth.

MACHTELD J. MELLINK
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FRED S. KLEINER and SYDNEY P. NOE. *The Early Cistophoric Coinage*. (Numismatic Studies, number 14.) New York: American Numismatic Society. 1977. Pp. 128. \$22.00.

Cistophoric coinage circulated widely in Anatolia in the second century B.C. Scholars have long regarded it as the product of quasi-federal municipal cooperation under the guidance of the Attalids. Sydney P. Noe left a study of this coinage incomplete at his death in 1969; Fred S. Kleiner, under the tutelage of Margaret Thompson, expanded and completed the work.

The first chapter reviews earlier theories about the inception of the *cistophori* and presents a new date, 166 B.C. The evidence is strong, but less than conclusive, and it should be remembered that coinage is not necessarily tied tightly to political events.

The bulk of the work is a catalogue arrangement of the coinage, illustrated by twenty-eight excellent plates. This does not allow illustration of all dies, so researchers will have to duplicate much labor or consult the author himself. Full illustration may not have been financially feasible, but even in its absence photographs could be used more efficiently: some illustrated dies are easily visible elsewhere, while many unillustrated dies are not. A short chapter deals with relevant hoards.

In the final chapter Kleiner draws together his conclusions. The royal Attalid mint of Pergamum struck cistophoric coinage in its own name, in the names of three other cities, and occasional special issues; Ephesus struck in its own name; Tralles for itself and probably for Laodiceia as well as most fractional coinage. This arrangement reveals clearly that the coinage is the product of Attalid design and purpose, a royal coinage without regal portrait or title. This is a significant contribution

to the studies of Attalid rule and the Hellenistic cities.

The work was composed in 1971. Minor revisions take into account literature until 1974, but not discoveries of coins. This has not affected the thesis or worth of the catalogue significantly, but an up-date article with supplementary plates would be welcome sometime in the near future.

Thanks are due to Bank Leu Ltd., Münzen and Medaillen AG, and the American Numismatic Society for funding this worthy publication.

DAVID J. MACDONALD
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E.-M. LAPERROUSAZ. *Qumrân: L'établissement Essénien des Bords de la Mer Morte: Histoire et Archeologie du Site*. Paris: A. and J. Picard. 1976. Pp. xii, 257. 120 fr.

E.-M. Laperrousaz, Director of Studies at the École Pratique des Haute-Études has written a contentious work setting forth his interpretations in opposition to Roland de Vaux, the director of the excavations of Qumran who passed away in 1971. The controversial nature of the book may be seen in the citation of 110 footnote references to twenty-one of his own publications, and of 1,033 (!) footnote references to twenty-eight publications by Père de Vaux.

The polemical purpose of the author skews his discussion and results in an unbalanced treatment of Qumran and the Dead Sea Scrolls. For example, the author devotes seventeen pages to the Copper Scroll but only fourteen pages to all the other Dead Sea Scrolls, which he simply lists without any discussion of their importance for biblical studies or for the history of Judaism and Christianity.

Roland de Vaux interpreted the crack running through Qumran as the result of the great earthquake of 31 B.C. and suggested that the Essenes had abandoned Qumran for forty years after this event. Suggesting instead that the Essenes had abandoned Qumran at an earlier date, Laperrousaz follows his mentor, A. Dupont-Sommer, who held that the Wicked Priest pursued the Teacher of Righteousness to Damascus between 67 and 63 B.C. His statement that this view is accepted by the majority of specialists (p. 219) is quite misleading; the identification of the Wicked Priest is a highly disputed subject and many more scholars would place the Wicked Priest in the period from 152 to 134 B.C.

The author sets forth the thesis that the deposits of animal bones found at Qumran do not represent the remains of religious sacrifices, but are rather the remains of feasts which were interrupted by

attacks by the Romans on two occasions, a little before 63 B.C. and again in A.D. 68. As de Vaux pointed out, this would have been a most remarkable coincidence.

Some cogent reasons are presented by Laperrousaz for holding that the two hundred tons of treasures listed in the Copper Scroll may have been actual objects rather than symbols. On the other hand, his view that the treasure was accumulated by the messianic pretender Bar Kochba (A.D. 132–35) is highly improbable and is not supported by any of the Bar Kochba texts recovered from Muraba'at and Nahal Hever.

In his 1959 Schweich Lectures, published in French in 1961 and translated into English in 1973 after his death, de Vaux had offhandedly dismissed the proposals of Laperrousaz with a brief comment: "It is not possible to discuss these hypotheses, which distort the evidence of stratigraphy, ceramics, and numismatics in the use they make of them" (*Archaeology and the Dead Sea Scrolls*, p. 22). It is a pity that de Vaux is no longer alive to respond to criticisms. Then again, even if he were alive, he still might not take the proposals of Laperrousaz very seriously. Indeed it is probably the *raison d'être* of this tedious tome to show the world that despite the disdain of the doyen of Qumran archaeology, the theses advanced merit consideration. Specialists may benefit from the realization that de Vaux was not infallible and did change his mind. Historians, on the other hand, are well advised to read de Vaux rather than the work of his tenaciously persistent critic.

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THOMAS KELLY. *A History of Argos to 500 B.C.* (Minnesota Monographs in the Humanities, number 9.) Minneapolis: University of Minnesota Press. 1976. Pp. viii, 214. \$15.75.

Thomas Kelly's well-written and closely argued study challenges many widely held views about the place of Argos in the history of the Peloponnesus during the archaic period. The author discards much of the literary evidence as late, fragmentary, and conflicting. Relying on archeological evidence and on Herodotus, he presents a new picture, not only of Argos but also of Sparta and Sicyon, that will surprise more traditionalist historians. In the first two chapters he presents an excellent synthesis of the archeological evidence for early Argos and the Argive plain. He uses this evidence to argue that the early history of Argos has been distorted since antiquity, there being no evidence to support the view that dark-age Argos presided over an empire. Rather, Argos was a small, self-

sufficient community with little influence beyond its immediate surrounding. In the eighth century, the author argues, Argos shared with the rest of Greece a quickening of life and rose to prominence in the Argive plain. Argive interests did not extend further afield, however, and by 700 the region was of little significance.

Of greatest interest are Kelly's discussion of seventh-century Argos and his excursus on the date of Pheidon. The view that Argos was a military power that wrested hegemony from the Spartans in a great battle at Hysiae in 669 does not square with the evidence. Sparta did not yet possess hegemony, and Argos was unimportant. The two states shared no common border, and the road between them ran through Tegea. Border conflict is unthinkable until after the Spartan conquest of Tegea in the early sixth century. The tradition that Argos was prominent in the development of hoplite warfare rests on no more than the fact that the lower acropolis bore the name Aspis. Ancient and modern historians alike have been misled by the tradition about Pheidon and his date. Kelly reviews the evidence thoroughly and argues that we must follow Herodotus in thinking that Pheidon lived at the time of Cleisthenes of Sicyon in the late seventh or early sixth century.

The study concludes with two chapters devoted to the reconstruction of Argive history in the late seventh and early sixth centuries with Pheidon so dated. His usurpation of the Olympic games is to be dated to this time and viewed as a political, not a military, act, aimed against Sicyon, not Sparta. After Pheidon, kingship at Argos shortly came to an end. The Spartan defeat of Argos in the Battle of Champions at Thyrea about 546 was a major turning point, permanently diminishing Argive influence in the Peloponnesus. Even so, the traditional enmity between Argos and Sparta is a fifth-century phenomenon, and it was not until 421 that the recovery of Cynouria became the motivating force behind Argive foreign policy.

This is an excellent book—thoroughly researched, fully annotated, carefully and concisely argued. It demands the most serious attention from all students of archaic Greece.

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R. A. TOMLINSON. *Greek Sanctuaries*. New York: St. Martin's Press. 1976. Pp. 150. \$16.95.

Religious sanctuaries were ubiquitous in ancient Greece, ranging from small and obscure to large and famous, such as Olympia or Delphi. As a repository for offerings and the site of cult and

ritual, the sanctuary is exceedingly important for our knowledge of ancient Greece—its history, religion and civilization. Nevertheless, no major study of the sanctuary as such had been published prior to this brief volume.

More than a third of the total text is a general description of the sanctuary, including a chapter on the buildings which is in effect a stimulating and up-to-date short history of Greek architecture. A very interesting discussion on financing follows. The remainder of the work contains short descriptions of what the author evidently considers to be typical Greek sanctuaries.

He divides his samples into four chapters: Major Sanctuaries (Olympia, Delphi, Delos); Other Important Sanctuaries (Athenian Acropolis, Argive Heraion, Isthmia, Epidauros); Lesser Sanctuaries (Aphaia sanctuary on Aegina, Nemea, Brauron, Perachora, Herakles sanctuary on Thasos, Arcadian sanctuaries); East Greek sanctuaries (Heraion at Samos, Artemis at Ephesos, Apollo at Didyma, Artemis at Magnesia). All these sites are discussed in some 85 pages with 46 plates of site photos by the author and 26 new plans and drawings, including two large fold-out plans of Olympia and Epidauros. There are brief chapter notes but no bibliography.

Inevitably, the reader familiar with the subject will disagree with some of the sites chosen for discussion and with what obviously has to be left out in such short descriptions. The exclusion of Eleusis is a major omission, at least for this reviewer. As one of the most important sanctuaries in the ancient world, it surely should have been described. At the other end of the spectrum, some mention of the small sanctuaries which were so typical of the ancient Greek city would have been welcome. Hero shrines are left out of the discussion, though they would seem to conform to the broadest definition of a sanctuary. This is a pity for new finds have greatly enlarged our knowledge of these strange, if typical, examples of Greek religious establishments. No sanctuaries in Italy or Sicily are discussed, but a whole chapter is given to those in the East. Chapter seven includes a section on Arcadian sanctuaries, which are hardly well known or typical, and seems to be principally a discussion of the unusual features of the temples in this part of Greece. This material would have been more appropriate as a separate article.

The notes have obviously been cut to a minimum, but perhaps in such a condensed study they should be more detailed. References to Greek publications are generally avoided, except in one or two instances, and this leads to unintentional misrepresentations. In the discussion of Brauron, a site which has not been generally available to English readers, the sole note lists only two references

to the English *Archaeological Reports*, thereby ignoring the numerous Greek excavation reports and even a popular survey in English in *Scientific American*. Statements which the reader will take as fact, but which are questionable, are often not footnoted. An example of this is the statement that there was a "door in a side wall" (presumably the cella wall) of the Temple of Zeus at Nemea (p. 131). The proof of this statement should certainly be given, especially when it would appear to be contrary to published discussions of the building.

The reader is in for a number of surprises, perhaps caused by excessive haste in preparing this work. The archeologist will be startled to read that in the fifth century B.C. "Corinthian pottery was no longer made" (p. 23) until he finds his way to note nine (p. 142) which adds, "Except for plain wares used locally" (*sic*). The classicist will similarly be bemused to find that in Homer "Hera favors the Trojans" (p. 12).

In short, this is a curiously unsatisfying book which, despite some excellent chapters, seems somehow thin in content. As the only major attempt to cover this important subject, however, it will be useful as a basic text and as a base from which to work.

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R. M. OGILVIE. *Early Rome and the Etruscans*. (Fontana History of the Ancient World, number 1.) Atlantic Highlands, N.J.: Humanities Press. 1976. Pp. 189. \$13.00.

R. M. Ogilvie enjoys a great reputation among classicists for his work on early Roman religion, on the sources for early Roman history, and more especially for his magisterial commentary on Livy, Books 1-5. The book at hand was first published last fall in Britain both in hard cover and in paper, because it is the first in a new series intended for a general public. If this first volume is any indication, the series promises to attract readers both in and out of the classroom. Ogilvie's work surely invites use in an undergraduate survey of Roman history.

Basically Ogilvie treats Rome in the sixth and fifth centuries and particularly deals with the problems of our sources, both written and archeological, and with the peculiar and imperfectly known symbiosis of Latins and Etruscans at Rome. Ogilvie offers a guide to understanding the sources and a judicious synthesis of recent scholarly publication on this history. He has accomplished the feat by purging his discussion of the rancor characterizing much of the work of his im-

mediate predecessors. Although it provides a synthesis, this book shows independence of judgment as well as some new interpretations. It remains, however, a history of Rome with an eye peeled for internal and external Etruscan influences.

More or less the author follows what we call the traditional chronology based upon the belief that the kingship ended and the annual magistracies began in last decade of the sixth century (see pp. 79–86). Throughout the history of the fifth century he puts great faith in the pontifical archives identified as the *annales maximi*, a source each of us has to pinpoint for himself while accepting the consular pairs. At the end of his period Ogilvie alerts his readers to the probability of a variant chronology which has greater claim to validity, namely Polybius' (1.6) synchronism for the year of Rome's fall to the Gauls. How Polybius came to know so roughly a precise date has never been thoroughly examined. Polybius' date is perhaps no better than the Romans'.

Amid much lucid explanation and exposition the author's account of the censorship (pp. 133–36, 151–52) and handling of the nature of the strife between patricians and plebeians (*passim*) seem inconclusive or misleading. Otherwise his treatment of the Decemvirate and its aftermath (pp. 111–36) is remarkably keen.

The period of this book coincides with that covered by Livy's books Ogilvie chose to comment on twelve years ago. In a sense this history is a guide to Ogilvie's Livy. Hence it ends with the fall of Rome. Yet many of the Roman problems belonging to the period reached their first substantive resolution in 367 and 366. Ogilvie should have abandoned Livy's dramatic periodization and extended his study to the historical conclusion twenty years after the fall of Rome.

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FRIEDRICH LOTTER. *Severinus von Noricum, Legende und historische Wirklichkeit: Untersuchungen zur Phase des Übergangs von spätantiken zu mittelalterlichen Denk- und Lebensformen*. (Monographien zur Geschichte des Mittelalters, number 12.) Stuttgart: Anton Hiersemann. 1976. Pp. vii, 328. DM 130.

The first half of this book is devoted to the general thesis that Eugippius' *Vita Severini* can only be properly evaluated if it is seen as a typical "early life," displaying well-defined hagiographical methods and assumptions. Friedrich Lotter puts forward a "hagiological" analysis which he himself likens to New Testament criticism, and in which "*typologische Stilisierung*" plays a larger part than adherence to historical fact. This part of his argument is hard to criticize, for it is conducted at

the highest level of theory and polemic; nevertheless, it is fundamental to Lotter's identification of Severinus, as we shall see.

Lotter's most conspicuous contribution to the study of Severinus is to identify him first with the *vir illustrissimus Severinus* mentioned in Ennodius' *Life of Antony of Lérins*, chapter nine, and then with the Severinus who was consul in A.D. 461. Since the *Vita* gives no inkling that the saint was a Roman aristocrat, let alone a consul, and indeed says in the first few lines that he had come from the East to Noricum in the 450s, this thesis might seem hard to sustain. But Lotter argues that the opening chapters (which contain virtually the only seemingly solid historical information about Severinus in the *Vita*) are the result of hagiographical rearrangement of material to fit into certain set ideas about the chronology of saints' lives. F. Prinz (*Deutsches Archiv*, 25 [1969]: 531–36) has already remarked that such treatment of the text seems cavalier to say the least. But there is a more serious objection to the identification with the consul of 461. Lotter supposes that the consul fled to the East on the fall of Majorian, and only later (467) found his way to Noricum. But the consul of 461 was certainly in Rome between 476 and 483, when his name was inscribed on a seat of the Flavian amphitheatre (CIL VI.32206; see *PLRE* 2 s.v. Severinus 5). This man, already influential in Roman politics in the 450s, can hardly be the subject of the *Vita*. As for the latter, if we accept the identification with Ennodius' *vir illustrissimus*, and assume (which is less certain) that the title does imply office, he might indeed have come from the East in the 450s. Two Severini were prefects of Constantinople in 398–99 and 423–24 respectively (*PLRE* 1 and 2, s.v.v.); quite possibly members of the family had migrated to the West for reasons we can only guess at. In any case there were numerous Severini, including several sons of the consul of 461. It would appear that there was some mystery surrounding our Severinus' origins that the saint himself exploited (*Letter to Paschasius*, cc. 8–9); but Lotter's solution would only fit if we were willing to follow him in doing violence to the interpretation of the text of the *Vita*.

Appeals to the logic of hagiography should not be used to justify ignoring straightforward historical fact. Despite Lotter's clever book, therefore, Severinus is still "keeping us guessing."

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ZBIGNIEW BUKOWSKI. *Elementary wschodnie w kulturze lużyckiej u schyłku epoki brązu* [Eastern Elements in Lusatian Culture at the Decline of the Bronze

Age]. (Polska Akademia Nauk, Instytut Historii Kultury Materialnej, number 37.) Summary in English. Wrocław: Ossolineum. 1976. Pp. 183. 45 Zł.

This book deals with the incursions made by steppe nomads into East Central Europe from ca. 1300 B.C. to the Scythian era. The author concludes that these infiltrations were concentrated in three waves during the Late Bronze Age and Early Iron Age. On the basis of a detailed typological analysis, Zbigniew Bukowski convincingly demonstrates that artifacts recovered in Poland have close parallels to those found in the lower Volga and north Caucasus. Materials that are markedly eastern in character are classified into three periods:

The first wave, ca. 13th–12th centuries B.C., is largely recognized by the distribution of the so-called Sosnovaja Maza swords that the steppe nomads brought with them. Three swords of this type have been found on the River San; however, no settlements or graves representing the first wave have been discovered in Poland. The author sees the connection of eastern influences in Poland with that in the Carpathian Basin. In Romania, particularly in Transylvania and to a lesser degree in the lower Danube region, steppe influences contributed heavily to a new cultural complex, the Noua group named after the eponymous site near Brasov in Transylvania. North of the Black Sea, this complex is known as the Sabatinovka group. It is considered to be the final Bronze Age phase of the Timber Grave culture that occupied the steppe region north of the Black Sea.

The second wave, ca. 9th–8th centuries B.C., is represented by numerous finds in the southwestern Lusatian area that have close affinities with materials produced in the Volga River basin, Kuban in the north Caucasus, and western Siberia. These include daggers, spearheads, battle-axe-shaped scepters, knives of a Siberian type, and horse bridle cheek-pieces. Until now, this wave was believed to be Cimmerian or Thraco-Cimmerian in origin. The author points out that there is no material basis for this assumption. Moreover, the presence of these artifacts in the Carpathian basin, Hungary, and Czechoslovakia leads Bukowski to the conclusion that this was the route steppe nomads followed to Poland.

The third wave is regarded not so much as a new movement of Eurasian nomads but as representing the maturation of influences introduced by the second wave. This is manifested in a local Carpatho-Danubian style which the author refers to as a pre-Scythian phase. In the territory of the Lusatian culture, these finds are found sporadically in southern Poland, in Czechoslovakia, and in the upper Dniester basin.

A spectrographical analysis of all eastern metal types is presented at the end of the text.

This book is a useful and up-to-date summary of eastern influences affecting the Lusatian culture. Bukowski's research demonstrates that the movements of steppe nomads did not stop in Hungary, but pushed further north into southern Poland. The intrusions of steppe nomads during the 13th–12th centuries and 9th–8th centuries B.C. were certainly not the first nor the last of the incursions that plagued Europe periodically.

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MICHAEL HERITY and GEORGE EOGAN. *Ireland in Prehistory*. London: Routledge and Kegan Paul. 1977. Pp. xvi, 302. \$21.50.

This is the first comprehensive survey of the prehistory of Ireland since the publication of Raftery's *Irish Prehistory* in 1951. Many developments have taken place in the interval in terms of both the interpretation of archeological material in relation to ancient societies and, more importantly, the establishment for the first time of a secure chronological framework through the techniques of radio-carbon dating. Raftery's account was a general one and very personal in its interpretation of the material. The present work is intended for undergraduate courses but is much more detailed and comprehensive than any previous account.

Following a brief survey of the hunter-fisher background, where already one can see a divergence in the Irish material from that elsewhere in the British Isles, the first major chapters are devoted to the early stone-using agricultural communities, whose most impressive surviving material remains are the great series of megalithic tombs, over eighteen hundred of which are still extant. The earliest of these are the court cairns concentrated in the north of the island. The authors would see the most elaborate of these tombs in the west as being primary, followed by an eastern spread represented by cairns covering simpler burial chambers. The evidence from Britain, however, indicates a development of chambers from simple to complex and their suggested late date for the portal dolmen type of tomb is again at variance with interpretations from the other side of the Irish Sea.

The appearance of settlers from Britain manufacturing Beaker pottery at the end of the Neolithic period raises problems in regard to the recognition of distinct social groups. In Western Europe generally, Beaker pottery is associated with a discrete cultural assemblage, including single-grave burials

and archers' equipment. In Ireland the picture is more complex in that Beaker communities appear to have been absorbed more readily and effectively into the native Late Neolithic population. Similar difficulties in interpretation are associated with Early Bronze Age material. Does the great variety of ceramic forms produced during this period characterize distinct social groups? The problem is perhaps greater in Ireland since much of the material, ceramic types in particular, appears to have been received from Britain. This same period, however, sees the beginnings of Irish supremacy in bronze technology in northwestern Europe, a position Ireland was to maintain throughout much of the second millennium B.C. Nowhere is this more apparent than in the Late Bronze Age when Irish smiths became masters in the working of sheet metal for the production of buckets and cauldrons and in the manufacture of some of the finest gold jewelry in barbarian Europe.

The concluding chapter, on the Iron Age is the shortest and perhaps the least satisfactory. This is due in part to the lesser amount of excavation and research generally devoted to this period and in part to the difficulty of establishing a precise chronology. It is surprising, however, that more use was not made of the great body of epic literature first written down in the Early Christian period but representing much earlier oral traditions which provide a rich mine of information concerning the society of the preceding pagan Iron Age.

In general terms the work succeeds in its aim of providing a detailed and up-to-date account of current knowledge of the prehistoric period in Ireland. The text is supplemented by a large series of line illustrations and maps, suffering in a number of cases from over-reduction, and by an excellent bibliography.

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MEDIEVAL

HERBERT GRUNDMANN. *Ausgewählte Aufsätze*. Volume 1, *Religiöse Bewegungen*. (Schriften der Monumenta Germaniae Historica, number 25, part 1.) Stuttgart: Anton Hiersemann. 1976. Pp. xxviii, 448. DM 120.

Herbert Grundmann (1902–70) was the president of the *Monumenta Germaniae Historica* from 1959 until his death. It is thus appropriate that the Monumenta has decided to issue a three-volume edition of his collected articles. This volume contains his studies about the religious life of the eleventh through fourteenth centuries; the later ones will

include his essays about Joachim of Fiore and medieval education. In spite of the similarity in titles, this book should not be confused with Grundmann's *Religiöse Bewegungen im Mittelalter* (1935). Only his supplementary report to the Tenth International Congress of Historians, which was published as an appendix in the second edition of that work (1961), has been reprinted here. These essays, which were written between 1927 and 1968, were either preliminary investigations or later elaborations of the themes Grundmann explored in that monograph.

Grundmann perceived the history of medieval Christianity as a conflict between the evangelical ideal and its ecclesiastical institutionalization. Whereas older scholars had rigorously separated Catholic orders from heretical sects, Grundmann stressed the common commitment by both groups to a life of evangelical perfection. What distinguished the saint from the heretic was not his life style or even doctrine, which was only of secondary importance, but rather his willingness to accept ecclesiastical discipline. If Grundmann had a hero, it was not St. Francis, but Innocent III, who succeeded in incorporating the new religious spirit within the Church. While the first devotees of the apostolic life generally belonged to the nobility, the movement gained its greatest support among the townspeople and especially among women. It was these women who provided the impetus for the development of German mysticism, which Grundmann saw in his studies of Meister Eckhart as an internalization of the ethic. Grundmann's conception of the basic unity of the religious life of the High Middle Ages has provided the analytical framework for most subsequent research in the field.

While Grundmann's East German critics, Ernst Werner and Martin Erbstößer, have erroneously applied the Marxist model of class conflict to the medieval idealization of poverty, there is no doubt that Grundmann neglected the sociological dimensions of the problem in his eagerness to demonstrate the spiritual motivation of the adherents of the apostolic life. He never explained, for instance, why the Cathars were so successful in Languedoc, whereas the reform movement remained essentially orthodox in Northern Europe. Similarly, Grundmann was satisfied to describe the people he studied as belonging to the upper strata of society, overlooking the fact that the differences between nobles, ministerials, and burghers were substantial. It is quite possible, for example, that the proliferation of Dominican nunneries in Germany was linked to the upward social mobility of the ministerials. While Grundmann correctly asserted that the presence of thousands of single women in the towns does not explain why they adopted a life

of extreme poverty, he paid little attention to the factors which caused this imbalance in the urban sex ratio. These comments are not intended to lessen the significance of Grundmann's work. It is precisely because he taught us to perceive the religious life of these centuries as a whole and thus also to see the differences more clearly that it is possible to make such criticisms. This book is a fitting tribute to a great scholar and a gentleman.

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MALCOLM LAMBERT. *Medieval Heresy: Popular Movements from Bogomil to Hus*. New York: Holmes and Meier. 1976. Pp. xvi, 430. \$29.50.

M. Lambert offers a general history of Western Latin popular heresy, diverging to include the Bogomils of Byzantium. A useful survey, the book is occasionally marred by excessive detail (see the chapters on Wyclif and Hus), but it has virtues, one of which is trying to deal with the major historiographical issues. One's heart is warmed when the author doffs his hat to Lea and Volpe, something few remember to do, but some of the historiography seems over-simplified. Norman Cohn is not to be credited, for example, with inventing the notion that heresy was lower class and urban. K. V. Selge does not deny that apostolic poverty meant much to the Waldensians: it was the basis of their attack on Catharism. What he wanted to emphasize was that the orthodox were particularly offended by Waldensian claims to apostolicity, the lay apostolate. Still, Lambert, an adherent of Herbert Grundmann's "intellectualist" line on heresy, is broadminded and kind to modern authors.

The author is not so kind to heretics, nor so perceptive about them. One is surely not surprised, for example, to read that, like most dualist gnostics, the Bogomils mocked the cult of the cross. To Lambert, this demonstrates "ill-instructed logical thinking at work" and a "total misunderstanding of the redemption." But, after all, many other than rural dualists believed that the orthodox doctrine of redemption was, or is, pure blither. Also, the author's treatment of the role of the sexes and sexuality among Latins of deviant belief is both conventional and naive. The curious Tanchelm, we hear, was a "libertine, who used his sway over women to satisfy his sexual needs." To describe the later Johann Hartmann as "probably a verbal exhibitionist" is to caricature not only the wonderful, if self-destructive, clarity of this deviant's argument, but also the good discussion in Lambert's own source, R. E. Lerner's

Free Spirit. Furthermore, some of the social history is not only conventional but, I think, manifestly wrong. It is not true that troubadour literature was sponsored "especially by noble ladies [ladies, yet!]," or that "partible inheritance by females created a fatal subdivision of rights in land" in southern France, or that, down there in the Midi, men were the "fringe members, the women the true converts" to Catharism. Besides, Lambert sometimes rushes in where angels fear to tread. "Exaggerations of poverty showed up elements of unhealthy thinking [!] in the whole Franciscan order," we are told, and, because of this extremism, "the way to needful reform [was] made harder by being associated with heresy."

One thing these curious observations about the Franciscans surely implies is that *a*, if not *the*, principal motive for heresy was dissatisfaction with the state of a Church in dire need of reform. Earlier on, we hear, the Lombard Patarins were driven to excess because of the "power of a simoniacal and unchaste upper clergy" who lived an "unregenerate life." One would never guess that Lambert is here really talking about what was in effect a married priesthood. Heresy was especially endemic in Italy because—shades of Marsiglio of Padua!—of the "political involvement of the popes," whose curial routines and fiscalism destroyed "opportunities for true reform." Nothing is more vapid than this progressivist and polemical rhetoric of "reform." It implies, as we see in medieval "reformers," modern Church historians, and, *mutatis mutandis*, contemporary Marxist social thinkers, that there is nothing wrong or insufficient in the original conception or institution. All that is needed is to restore man, or to advance him, to the perfection of the primal idea. To indulge in similar polemic for a moment, it is obvious that, from the age of the apostles until today, the Church has been constantly being "reformed." And just look at it now!

Polemic aside, I also think that Lambert is somewhat impeded by his excessive concentration on divergent belief or heresy enjoying fairly wide popular acceptance. Because they had no mass following, he purposely excludes Ockham and Marsiglio, the latter of whom, we hear, was "without a reputation or depth of learning like that of Wyclif." Apart from that White Cliffs of Dover crack, Lambert's decision is OK: one book can't treat everything. But the decision not to treat is not the same as to overlook or neglect intellectual and institutional elements vital for the subject. Intellectually, Ockham was among those who created what was to become the *via moderna* against which both Wyclif and Hus reacted to found their "heresies," and both he and Marsiglio, especially the latter, were among those "biblicalists" who

provided basic justifications for the *sola scriptura* arguments of later radicals. Institutionally, it is surely to be noted that, unlike most of their popular predecessors, the leaders of late medieval divergent and critical thought were, from the days of Ockham and Marsiglio or shortly before, leading ecclesiastical notables and teachers who were long protected by strong institutional affiliations, especially by secular princes. It is therefore not surprising that Luther, who was, although decidedly closer to the *via moderna*, as much of heretic as Wyclif and Hus, was supported by the early protection accorded him by his prince, even before the propaganda of the 1520s brought him popular adhesion. The people, it seems to me, do not always act directly, but often by means of the institutions and offices they create or permit to be created, in this case, by the governments of secular princes and kings.

But that is another story, and, in the meantime, Lambert's is a useful and honest survey of medieval popular heresy.

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CHRISTOPHER J. KAUFFMAN. *Tamers of Death*. Volume 1, *The History of the Alexian Brothers from 1300 to 1789*. New York: Seabury Press. 1976. Pp. x, 234. \$15.00.

There are important gaps in our knowledge of the religious history of the Middle Ages. The monastic orders of the early Middle Ages have long had their histories, the friars have been studied both in the lives of their founders and their later development, and even the canons regular have a distinguished, if less massive, bibliography. All of these orders have certain advantages which have attracted historians: vibrant personalities, such as Bernard or Francis, as founders, or an ancient tradition of scholarship to build on, as in the case of the Black Benedictines. But a number of late medieval communities, such as the Alexian Brothers, remain very little known.

The earliest Alexians were Beguines—pious, illiterate, and frequently accused of heresy. They developed during the fourteenth century from urban communities dedicated to the “apostolic life” of common property and good works. They lacked a single founder, and it was long before they developed even a loose confederation. This lack of focus has left their history in almost total obscurity. In the last few decades, however, interest in the late Middle Ages, especially in the topics of heresy and popular piety, has increased. The time would thus seem ripe for a scholarly history of the Alexians. Unfortunately, the time is not quite ripe, and

Christopher J. Kauffman's book is not that history.

Since the early Alexians have left us almost no testimony of their domestic history or spirituality, Kauffman has been forced to imagine what his prototypical early Alexian “must have thought.” Lacking carefully edited cartularies, he is obliged to refer to the archives of communities in Aachen and Antwerp without being able to check their authority or authenticity. When even these slender resources fail, material from distinguished but not totally reliable authorities such as Huizinga and Ariès is called upon to fill the gaps.

Very little of this can be blamed on Kauffman. If he is constantly obliged to assure us that certain events “must” have occurred, or that his imaginary typical Alexian “must” have reacted in a certain way, it is not because he wants to force the data, but because there is as yet no reliable data at all. The problem with this book is simple: it does not have enough tested information to provide a critical history of the Alexian Brothers. Its virtue is that it should inspire scholars to begin the painstaking work of assembling the sources which will some day make its replacement possible.

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ROBERT E. RODES, JR. *Ecclesiastical Administration in Medieval England: The Anglo Saxons to the Reformation*. Notre Dame, Ind.: University of Notre Dame Press. 1977. Pp. xvi, 287. \$19.95.

Robert E. Rodes, Jr. undertook the formidable task of writing an administrative and legal history of the English Church from the Anglo-Saxon period to the late Middle Ages in 203 pages of text and 65 pages of notes. Within those limits he has treated the Anglo-Saxon Church: the Gregorian Reform; canon law; provincial, diocesan, and parish structures; and the papacy. The text is necessarily compressed, although rarely to the point of being a mere catalogue, and it presupposes on the part of the reader a general familiarity with medieval English history. The author relies heavily on secondary works and on learned encyclopedias such as the *Dictionnaire de droit canonique*. Specialists in various facets of Church history will regret the absence of references to the monographs and articles which could have made the text deeper and more precise at certain points. The author's primary focus is the thirteenth-century Church, and he moves back or forward in time as topics demand. Such a procedure reinforces the author's view of basic continuity in the elaboration of ecclesiastical institutions between the eleventh and

fourteenth centuries, but it blurs perception of the great changes in institutions and *mentalités* which separate Gregory VII and his handful of curial scribes from John XXII's Avignonese bureaucracy. Finally, the author empties the term "Gregorian Reform" of its precise meaning by using it to encompass virtually every form of moral, economic, legal, and administrative improvement in the Church from the eleventh to the fourteenth century.

The author's considerable strengths lie in his insight and mastery of synthetic statement. He is skilled at taking well-worn material and rethinking it so as to gain new perspectives. He is independent in his judgments, which are both concise and incisive. In particular the thirty-two pages on the "Gregorian Reform" are a well-organized and thought-provoking introduction to the aims of orthodox medieval Church reformers.

In recent decades, the thought of the medieval canonists has received much attention from scholars, who too often accept the learned commentaries as descriptions of reality. Rodes' book is a corrective to such academic innocence. He argues that, when viewed as a legal and administrative system that actually had to function, the medieval Church was often weak, indecisive, and ineffective. The Church's religious mission interfered with its bureaucratic efficiency: the erring Christian was viewed as a sinner to be led to repentance rather than as a criminal to be punished. This pastoral concern occasioned the delays, appeals, repetitions, fierce threats, and mild punishments which are so characteristic of ecclesiastical legal and administrative proceedings. Even the papal monarchy emerges in a new light when judged by Rodes against the measures of effectiveness and purposiveness: "A romantic conception of pastoral solicitude, a utopian conception of the *plenitudo potestatis*, and an epic bureaucratic inertia had combined to embark the papacy on administrative functions it lacked (a) the power, (b) the money, and (c) the information to carry out effectively" (pp. 200-01).

This is a thoughtful book, which examines traditional themes in a refreshing, even iconoclastic way.

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H. C. DARBY. *Domesday England*. (Domesday Geography of England.) New York: Cambridge University Press. 1977. Pp. xiii, 416. \$45.00.

Since the late nineteenth century the folios of Domesday Book have presented a fascinating and continuing challenge. Every generation of histo-

rians has produced its Domesday scholars, notable among them Maitland, Round, Stenton, Douglas, Galbraith, and, more recently, Welldon Finn. Their concern, in the main, has been with social and legal matters. It was not until the 1930s that H. C. Darby, then at Cambridge and using the information in Domesday Book for his work on the medieval Fenland, conceived and set in motion a geographical analysis of the Domesday material for the England of 1086, a formidable exercise that has become known as the Domesday Geography of England. With hindsight the mapping and regional analysis of the data of the folios would seem to have been an obvious step, but perhaps we need to remind ourselves, as Postan has done, that such an approach to Domesday Book, and the resulting insights, would probably not have occurred to most social and economic historians at that time.

Despite the interruption of the war years Darby's first regional volume, *The Domesday Geography of Eastern England*, was published in 1952. With the help of a team of contributors and co-editors there followed *Midland England* (1954), *South-East England* (1962), *Northern England* (1962), *South-West England* (1967), and, in 1975, a *Domesday Gazetteer*. For ten years we have been anticipating Darby's final overview, *Domesday England*, and 1977 turns out to be the silver jubilee of both Elizabeth II and the Domesday geographies. More important, publication of the volume marks the culmination of over forty years of work, by pre-computer methods, on the Domesday Inquest.

In this volume a jigsaw has been put together from the pieces of the five preceding regional studies. While readers interested in the Domesday evidence for particular English counties will still turn to the detail of the regional volumes, they are now able to see how local evidence fits into the picture for the country as a whole. After a brief introduction to the Domesday Inquest, the information on England is presented through eight thematic chapters (in contrast to the county chapters of the regional volumes) covering rural settlements, population, arable land, pasture and livestock, woodland and forest, annual values, industry, and towns. Additionally there are chapters on devastated land and the Welsh March. There are twenty-one appendixes, most of them either summary tables of Domesday statistics or lists of references in the folios to particular types of land use and economic activity—marsh, forests, vineyards, markets, and mints. As in the regional volumes every opportunity has been taken to present data cartographically; there are 111 maps, all drawn by G. R. Versey, whose contribution to the series has been a notable one both through his cartography and his co-editorship of the *Domesday Gazetteer*. His maps are models of clarity, and it is therefore

unfortunate that through a number of blemishes the blockmaker and publishers have not always done them justice, perhaps, one suspects, as a result of trying to minimize costs in some way.

Domesday Book, 1086, gives us, of course, only an incomplete picture of late eleventh-century England. Despite its remarkable coverage (all of England except the four northern counties) and the statistical uniformity for its date, it is undoubtedly weak on certain aspects of English life, in particular on the towns, and on the settlements and economies of the uplands. While it lists over 13,000 place-names (three percent of which have still to be identified) many places which are known from other evidence to have been in existence by 1086 are not named. Additionally, and unfortunately, it tells us nothing about the form of rural settlements, a controversial question which is now occupying much space in English publications on historical geography and medieval archeology. Moreover, the picture it presents is a static one which has often been compared to a still from a movie; we know little of the story that precedes it even though the abundant documentation of later medieval England helps us to fill in some of the sequels. Darby is aware of all these drawbacks; Domesday Book, as he says, "bristles with difficulties," and he devotes considerable sections of this book to the imperfections of the data. Nowhere is this more true than in the vexed question of the values, which, having been omitted from the earlier volumes, is discussed here. Having said this, however, Domesday Book, even as a "still" has a lot to offer; Darby stresses its positive side, its remarkable information on the categories of rural population and their distribution, on woodland, plough-teams, meadow, and activities such as saltmaking and ironworking. Distributions are discussed with particular reference to the environmental variables of soil and terrain; less attention is paid to the legal, fiscal, and administrative complexities, aspects of which continue to be the research interest of many historians.

A past criticism of the project has been that Darby has concerned himself solely with the Domesday evidence and not with other near-contemporary sources for late eleventh-century England. So be it. With the Domesday geography completed, this work can now be incorporated into studies of wider issues such as the organization of medieval society, feudalism and its modes of production, medieval settlement, and colonization. Darby's work is completed just as a new generation of geographers is putting Domesday data on computer. No doubt this will produce some new insights into certain aspects of the folios and this is as it should be. Domesday scholarship has never stood still, and Darby would be the first to say that

his work has raised more questions than it has answered. Until some of these are resolved (many never will be) *Domesday England* will remain a landmark.

In this book, fittingly published a few months after his retirement from the chair of geography at Cambridge, Darby includes a fascinating personal account of the writing of Domesday geography (appendix 21) in which he describes the long haul and the reactions of reviewers to the earlier volumes. We must be glad, with him, that his heart did not fail at the enormity of the task he set himself over forty years ago.

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FRANK BARLOW *et al.* *Winchester in the Early Middle Ages: An Edition and Discussion of the Winton Domesday*. Edited by MARTIN BIDDLE. (Winchester Studies, number 1.) Oxford: Clarendon Press. 1976. Pp. xxxi, 612. \$52.50.

This handsome, costly book is the first in a series of volumes that will combine the results of archeological and historical investigation into the English town of Winchester from its pre-Roman origins to the present. The excavations directed there by Martin Biddle have already received much attention. Now the results are combined with an edition and translation of the Winton Domesday and with an analysis of the personal and place-names that give an unparalleled picture of one of England's most important boroughs in the twelfth century.

The Winton Domesday is edited—with scholarly notes—and translated by Frank Barlow. Like London, Winchester was not included in Domesday Book. Its survey consists of two separate descriptions of the borough, both contained in a single manuscript. The first was taken ca. 1110, demonstrably based upon a survey of the mid-eleventh century. It may be, as Barlow suggests, that Winchester was excluded from Domesday Book "only because the task of transferring one set of records to another was never completed" (p. 9), but the very fact that Henry I needed to take his own survey makes more plausible Galbraith's earlier assumption: that Winchester and London presented the royal commissioners with too great a task for the short time they had available, even with Old English surveys already to hand. Henry I's survey consists of a list of royal lands in the borough, one of the long series of investigations by means of which commissioners of the crown tried to keep track of royal rights and revenues in the Middle Ages. The second survey was carried out in 1148/49 under the active administrator Bishop Henry of Blois. This time the entire borough was covered. Both were printed in 1816, in volume four

of the Record Commission *Domesday Book*, along with other satellite texts.

Winchester was an ancient city, already continuously occupied for over a thousand years when the Winton Domesday was taken. Its messuages lay within a Roman wall, and some of its approach-roads were Roman. But the Winchester of this text and of the excavations that support it, is the great borough of Wessex. The Roman town was long gone; the interior street plan is revealed as of the late ninth or early tenth century, the result of Alfred's policy of urban formation, the backbone of Wessex' successful defense against the Scandinavian invasions. The great days of the borough as a royal center lay between the reigns of Alfred and Stephen, when a royal palace was built to the west of the Old Minster and when Winchester became the permanent repository of the royal treasure and home of the royal administration. One of the first acts of the Norman conquerors was to build a castle there and a cathedral in the new style. But by the epoch of the 1148 survey Winchester's position as royal city was fading as the center of administration moved to Westminster. From 1148 through the remainder of the Middle Ages, its prosperity and population declined. The Winton Domesday catches the city at its last great moment, and this edition does scholarly justice to the administrative skill and care lavished upon his survey by the bishop whose city it had become. The thousand and more households are plotted on beautifully drawn maps and correlated with the available archeological evidence; the householders' names are analyzed with minute care by no less a scholar than Olof von Feilitzen. Fortunately for social historians, the section on social structure is brief and written by archeologists. For all its formidable bulk, the book leaves much still to be done; it gives historians an exceptionally solid base from which to proceed in their future work.

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CHRISTOPHER R. CHENEY. *Pope Innocent III and England*. (Päpste und Papsttum, number 9.) Stuttgart: Anton Hiersemann. 1976. Pp. xii, 433. DM 100.

When a great historian prepares a narrative account of a subject with which he has been occupied previously in more technical ways, there is every reason to expect a significant volume. The present work is just such a book. Christopher R. Cheney is well known, not only for monographs such as *From Becket to Langton* and *Hubert Walter*, but also for studies of the sources for Pope Innocent III's reign. The most famous work in the latter category is the

magisterial calendar and select edition of Innocent's letters to England and Wales, produced in collaboration with Mary G. Cheney in 1967 (but which is now, regrettably, out of print).

The book at hand is the first to appear of a group of volumes treating Innocent III, in the *Päpste und Papsttum* series (Raymonde Foreville is preparing a study of Innocent III and France; Michele MacCarrone will handle Innocent and Italy). Cheney's work has set a high standard for subsequent contributions on this important pontificate. His work has a tripartite division. An introductory section presents the *dramatis personae*—the pope, cardinals, English kings, and clergy—and also probes "contacts" ("By what means did the Curia keep in touch with the king, the clergy, and the people of England?" [p. 25]). Six chapters are devoted to the scope and mechanisms for Church government under Innocent (with emphasis, of course, but not exclusive attention, on England); six more chapters treat political issues which concerned Rome and England between 1198 and 1216. In his preface, the author points out that the distinction between parts two and three—ecclesiastical government, and politics—"is fundamentally unreal." Yet the division is sensible in terms of Cheney's concern to stress the importance of ecclesiastical government "in the stricter sense" as opposed to studies of Innocent's activities which have been much more occupied with politics. "By now so much erudition has been expended upon papal diplomatic and so many aspects of the English Church have been examined in detail that it should be possible to redress the balance" (p. viii).

Throughout the volume Cheney writes with a felicitous and incisive quality which recounts familiar things with ingenuity. Speaking of papal politics, for example, the initial paragraphs of part three note the tensions between theory and practice thus: "If politics is the art of the possible, it still remains true that to gain all that is possible it may be necessary to aim at getting much more. The medieval papacy consistently asked for more, immeasurably more. . . . Doctrine and logic required the pope to write as though all Christians would rush to obey or submit at once when censured. . . . Christians did not always carry compliance so far, and the pope knew it" (pp. 271-72).

Medievalists could hardly ask for a finer treatment of this book's subjects than the one Cheney provides.

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MARJORIE NICE BOYER. *Mediaeval French Bridges: A History*. (Mediaeval Academy of America, number 84.) Cambridge, Mass.: The Academy. 1976. Pp. xii, 236. \$12.00.

When a dozen years ago Marjorie Boyer described with such a wealth of detail the rebuilding of the medieval bridge at Albi one entertained the hope that other early French bridges might one day be similarly treated. In *Medieval French Bridges: A History* this hope is realized and splendidly so. Using an impressive array of manuscript and published sources, Boyer presents an account of the bridge in medieval France as rich in information as it is enjoyable to read.

The book considers the medieval French bridge from a variety of standpoints, all generally reflecting the nature and content of the sources. Thus, much of the book is concerned with the administration and planning of bridge-building, the raising of money, the exacting of tolls, and the financing of upkeep and repair necessitated by the frequent minor and major collapses which plagued medieval bridges. The author's assessment of the respective roles of Church and laity in funding and engineering the structures is especially valuable and interesting.

For the historian of civil engineering the study is a trifle bare on some issues (those matters not characteristic of the sources), but notably informative on certain unexpected topics. The most basic technical question still persisting is why the Middle Ages (and not just in France) often preserved the Roman style of masonry bridge-building (semicircular arches), occasionally took a significant step forward and used segmental arches (Avignon, twelfth century), and yet was just as likely, even at a comparatively late date, to regress to pointed-arch construction, the worst of all worlds in terms of volume of construction, foundation problems, and restriction of river flow. More rational structural configurations had to wait until the upsurge in bridge-building in Italy which began about 1400. Conceivably the explanation lies with esthetic ideals rather than constructional logic, a slavish devotion to pointed arches (very suitable for churches) only being abandoned when new architectural canons were accepted. Certainly the point substantially confirms Boyer's view that medieval bridge-building was a very parochial craft.

By contrast the sources have revealed many interesting points about constructional techniques—pile driving, coffer-dam and starling construction, the erection of falsework—and about the provision of materials and rates of work and pay. Moreover, while it is no surprise to learn that the stability of medieval bridges was at greater risk from foundation failure than from inherent defects in the superstructure, it is illuminating to see this spelled out in the historical record.

The bridge occupied a place in the social, commercial, technical, and cultural fabric of medieval

France (and Europe generally) which was in marked contrast to any other period, especially our own. Its history is graphically set out in Marjorie Boyer's book.

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MICHEL BUR. *La formation du comté de Champagne (v. 950-v. 1150)*. (Mémoires des annales de l'est, number 54.) Nancy: Université de Nancy II. 1977. Pp. 573.

The county of Champagne was in many respects one of the leading regions in France in the twelfth and thirteenth centuries, but the roots of that flowering have until now remained obscure in its earlier history. Michel Bur, of the University of Nancy, has rendered a singular service by collecting the disparate Champagne sources from Roman times to 1150 and presenting a coherent description of the political, religious, and economic developments that were preconditions to the later dynamic period. A comprehensive guide to the source materials and an extensive bibliography make this book indispensable to an understanding of medieval Champagne.

Since the purpose of this study is to trace the evolution of Champagne before it became a cohesive territorial state in 1150, much of the book is given to close political-genealogical analyses from the ninth century (there are thirty-four genealogical tables of the most important families, many of which had ties beyond Champagne). The rule of Count Eudes II (996–1037) was pivotal. Originally count of Blois, Tours, and Chartres, he inherited Troyes, Meaux, Vitry, and Châlons-sur-Marne from a cousin in 1021, thereby encircling the royal domain. The king's attempt to dispossess him elicited Eudes' letter of 1023, probably written by Fulbert of Chartres, justifying the collateral inheritance on the grounds of past loyal service to the Crown, as well as hereditary right. For Bur, that recognition of ultimate royal authority over the comital office disproves the recent thesis of a collapse in royal authority in the early eleventh century.

The count was without peer as layman in Champagne, as holder of substantially more land, rights, castles, and vassals than any other baron and as protector of the most important monasteries. Except at the distant peripheries, where a few castellans acted with impunity, there was no collapse of the count's authority over his barons. However, Eudes was forced to sell his share of the city of Reims to the archbishop in 1023, and successor counts were gradually forced out of Châlons. With

the loss of these two northern urban centers, the county became mainly rural and the counts fell back to Troyes, the only Roman city under their full control. The policy of Count Thibaut II (1125–52) that subverted the old Roman route from southern France to Reims and Châlons by enticing merchants to pass through southern Champagne laid the foundation for the international fairs that became crucial to the county's prosperity.

Beyond the regional context, the author has made an important contribution to the ongoing discussion about the nature and evolution of political authority in early Capetian France. Finding that the hereditary castellans in Champagne exercised their functions in delegation and were generally loyal to the count, Bur discounts any disintegration of comital power into the hands of "independent" castellans in the early eleventh century. He confirms recent studies from other areas that find a continuity of the county as the basic unit of government until almost the twelfth century; the collapse of the Carolingian *pagus* and the rise of independent castellans now appear to have been the exception, not the norm, in much of northern France.

THEODORE EVERGATES
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ANNA M. DRABEK. *Die Verträge der fränkischen und deutschen Herrscher mit dem Papsttum von 754 bis 1020*. (Veröffentlichungen des Instituts für österreichische Geschichtsforschung, number 22.) Vienna: Hermann Böhlau. 1976. Pp. 128.

Anna Drabek has undertaken to study the treaties concluded between the Frankish and German rulers and the papacy from Ponthion to the Henricianum. Although her work owes much to the diplomatic studies of Sickel, Stengel, and others—and even adds a little to their findings—she is, like Wolfgang Fritze, more concerned with the meaning of the documents' contents and with their legal character than with their form in the strict sense.

Her main thesis is that there were never two separate arrangements entered into by popes and rulers, but only one. Numerous scholars from Haller to Schramm have insisted that there were regularly two enactments: a personal relationship of some sort (disagreements abound here) and a promise to protect the Roman Church. Drabek subtly alters the nature of this old problem. She suggests that an "*amicitia-Bundnis*" lies at the base of the whole relationship and that *amicitia*, which one can characterize by the formula *amicus amicus inimicus inimicus*, brought in its train a duty to protect. The *amicitia-Bundnis* was sealed by mutual

oaths which were not written down. Obligations that flowed from the oaths were committed to writing and resulted in documents which she calls *pacta*. The extant *Ludovicianum* of 817 is the oldest surviving *pactum* but others were issued in 754 and 774. Thus, one can formulate: *amicitia-Bundnis* plus *pactum* equals *Vertrag*.

A few other points made by Drabek deserve mention. The obligations of the contracting parties were genuinely mutual, and the popes were not the beneficiaries of unilateral promises of protection. The papal obligation was, of course, essentially spiritual and is best described in Charlemagne's famous letter to Leo III of 796. But, consistent with the *amicus amicus* formula one must suppose that the popes were not to enter into alliances that would have been injurious to the Franks. Also, the later *promissio imperatoris* of the coronation *ordines* does not seem to derive, as Eichmann and others believed, from the *Verträge* initiated in 754 but from the political schemes of John VIII.

Though the book attempts to cover the period from 754 to 1020, the bulk of it consists of a detailed analysis of the years 754 to 817. This is justifiable to the extent that it was during these years that the basic nature of the treaty was worked out. However, the complex and important series of relationships between popes and rulers after 817 ought to have been analyzed more fully.

This is an impressively documented and carefully argued book on one significant and complicated aspect of papal-temporal relations, but in an area where there has been so much controversy Drabek is unlikely to have the final word. I am not entirely persuaded that there were not actually two different arrangements, and I have some questions about her insistence on the curial origins of the *Verträge*, since the *amicitia* institution was certainly more Frankish than Roman by the eighth century and as it was applied by the Carolingians. Nonetheless, the author is to be applauded for making an original and important contribution.

THOMAS F. X. NOBLE
Texas Tech University

LUTZ FENSKE. *Adelsoption und kirchliche Reformbewegung im östlichen Sachsen. Entstehung und Wirkung des sächsischen Widerstandes gegen das salische Königtum während des Investiturstreits*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 47.) Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 401. DM 85.

This book seems to have a dual purpose. The first is to analyze more carefully than Bosl, Baaken, and others the nature and course of the Saxon

revolts against Henry IV and Henry V. The second is to explore possible connections between the ecclesiastical reform movement and specifically aristocratic opposition to the crown. Lutz Fenske has evidently been inspired by the investigations of such scholars as Karl Schmid, Hans-Josef Wollasch, Kurt Hils, Heinrich Büttner, and Hermann Jakobs of the role of the nobility in monastic reform in southwest Germany in the eleventh century. Jakobs, in fact, goes so far as to speak of a *Reformadel*. Fenske is chary of such an abstraction, just as he hesitates to view the Saxon rebellions as those of the *Volk* or *Stamm* or to label antiroyal bishops "Gregorians" without further ado. This healthy reaction against what someone once called the "*Begriff-stricken*" character of much German scholarship results here in a carefully differentiated picture of the relationship between reform and rebellion in Saxony. Fenske generally tends to de-emphasize the nexus between the high nobility and the reformers, despite many points of contact between the two. Thus, although Bishop Burchard II of Halberstadt was able to pull together and lead many diverse factions until his assassination in 1088, aristocratic resistance to Henry IV had in fact begun to disintegrate soon after the death of Otto of Nordheim five years earlier. This was apparently related to the fact that Burchard and the other two leading reform-minded prelates (Herrard of Ilseburg-Halberstadt, also bishop of Halberstadt, and Archbishop Hartwig of Magdeburg) were non-Saxons and that the whole reform effort here bore a strongly southwest-German stamp. At the same time Fenske does not deny the development of indigenous reform and undertakes a detailed inspection of four aristocratic reform houses: Hillersleben, Reinhardsbrunn, Pegau, and Paulinzella (pp. 221-92).

These are some of the more discernible conclusions of a book which is most trying to read and ultimately unsatisfactory. Fenske rightly points out that many fewer sources exist for Saxony than for the southwest, and so he must squeeze them very hard. Questions of diplomacy and prosopography consequently occupy great stretches of text, arguments which would often have been better relegated to notes or separate articles. Fenske could have compensated for these limitations on readability by explaining to the reader frequently and succinctly his intentions and conclusions; but he does not. Instead one usually has a distressing feeling of not knowing the point of what one is reading, be it on a particular page or in general. The overall plan of the work and the progression of the chapters are not at all clear. One senses finally that, appearances notwithstanding, the author has not extracted from the sources all that they contain, and so we still await both a more straight-

forward narrative and a more thoroughgoing and lucid analysis of this admittedly complex problem.

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DIETMAR FLACH. *Untersuchungen zur Verfassung und Verwaltung des aachener Reichsgutes von der Karlingerzeit bis zur Mitte des 14. Jahrhunderts*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 46.) Göttingen: Vandenhoeck and Ruprecht, 1977. Pp. 431. DM 90.

The title of this book fairly represents its contents. Included in it are studies, interrelated but often poorly integrated, on topics relating to the organization and administration of royal lands and rights in the area of Aachen from the ninth to the fourteenth centuries. It is a dissertation, written under Walter Schlesinger and accepted at Marburg in 1974, and bears the strong imprint of a dissertation, even at times of a series of seminar papers strung together. The reader would be well advised to begin the book with the conclusions, and then to search out the summaries of individual sections that come earlier. (Unfortunately these are not evenly distributed throughout the book.) Only then do some of the more detailed sections make sense.

Dietmar Flach regrets (p. 94) that inadequate sources force him to adopt a "retrospective method," the dangers of which he recognizes. Sources from later centuries are studied to shed light on jurisdictions of earlier and more obscure periods. There are grave dangers in this of misconstruing the earlier jurisdictions, their competence, or the officials and their sources of income. But the most serious problem is that rule and jurisdiction end up like so many concrete objects passed down the centuries from kings and emperors to *Vögte* and *Schultheissen*, to dukes and towns.

The book should be of great value to anyone seeking information on subjects such as the competence of the offices of the *Schultheiss* or the *Vogt* in Aachen or what we know about the individual *Schultheissen*. Given the nature of the sources Flach is using, there will doubtless be further historical controversy on individual points, but Flach uses the available sources well. He seems skilled in exploiting the content and the context of a passage and solves individual problems of interpretation in a convincing fashion. He is well versed in the secondary literature; at times the scholarly debate even threatens to overwhelm the text.

The best sections of the book discuss historical geography: the location of buildings and areas in Charlemagne's Aachen, the limits of the *villa* of Aachen and its neighboring *villae* in the ensuing

period. Useful, indeed indispensable, maps complement these efforts at reconstruction. The worst sections of the book are those that touch on social and economic issues (as opposed to fiscal administration). The concluding section, on the development of urban life and urban institutions, is for the most part perfunctory.

In general Flach is most successful when he has a document in front of him to analyze or when a series of more specific references can be evaluated, arranged in order, and summarized. He is least successful when he attempts to synthesize his individual observations into clear and significant conclusions.

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DAVID ABULAFIA. *The Two Italies: Economic Relations between the Norman Kingdom of Sicily and the Northern Communes*. (Cambridge Studies in Medieval Life and Thought. Third Series, number 9.) New York: Cambridge University Press. 1977. Pp. xvii, 310. \$32.50.

The substance of this volume is best described by its subtitle. Yet the title does stress the underlying theme of this study which, briefly stated, is the following: A pattern of commercial dominance of the markets of southern Italy by northern merchants began to develop in the middle of the twelfth century. By the end of the same century this trend became irreversible, leading inevitably to the economic colonization of the South, which became an agricultural-pastoral area furnishing to the northern communes foodstuffs for their teeming populations and raw materials which provided jobs for their workers.

In the first part David Abulafia explains his research plan, which is based on an innovative and resourceful use of the large collections of notarial acts found in the archives of Genoa and other northern communes. These cartularies, so far mainly exploited to investigate the trade of Genoa, Pisa, and Venice in the Eastern Empire, the Levant, Egypt, and north Africa, contain large numbers of acts which refer to the trade with the Norman kingdom. Until now, in the total absence of contracts or notarial collections, the commercial history of the South has been based on traditional sources such as chronicles, grants, charters, and any information concerning economic matters that could be gleaned from contemporary literature like lives of saints, *Miracula*, and so on. The problem with the data found in such sources is that they do not lend themselves to useful statistical evaluation.

In the second chapter of part one Abulafia examines the resources of this trade. The Norman kingdom of Sicily was uniquely important for

northern traders because it was a source of urgently needed staples, particularly for land-starved and rapidly developing Genoa and the other communities along the narrow Ligurian coast. Moreover, Sicily was a busy midway market for Eastern spices and the luxury trade and, alternatively, a support base for the long voyage to Egypt and Syria. Import of foodstuffs was facilitated by the existence of a very large wheat-producing royal demesne, whose crops the kings of Sicily were happy to dispose of for cash.

Part two, which deals with the development of this trade, examines the constant preoccupation of the northern communes to secure trading privileges in the *Regno*. Especially interesting are the changing positions of Genoese diplomacy toward the Normans, as the republic tried to balance its political interest, which drew it to the imperial side, against the interests of its merchants and the need for Sicilian wheat. Abulafia's statistical tables on the volume of trade with Sicily, compiled from the cartularies of Genoese notaries, faithfully reflect the fluctuations in diplomatic relations between the republic and the Norman kings. The pact of 1156, for instance, started a great surge in the volume of trade with Sicily, a response that the author, momentarily abandoning his reserve, calls "enthusiastic indeed" (p. 108). Conversely, when Genoa shifted to Barbarossa's side in 1162, the cartularies show that trade with Sicily practically disappeared in 1163-64.

Part three, on the structure of trade, discloses, again through careful use of the statistical evidence in the cartularies, the dramatic increase in the export of textiles to southern Italy. Whereas in the 1150s and 1160s these were limited to fustian and some cloth coming from Milan and Monza, in the 1180s and 1190s the main exports were high-demand, expensive woolens from Flanders and silk and scarlet cloth from Lucca, in much larger quantities. Abulafia postulates that this shift caused a deficit in the balance of payments of the South (excluding Sicily). Yet for lack of statistical evidence this conclusion must remain, in his words, "impressionistic only."

Space limitations do not allow the full discussion this work deserves. It is a major scholarly contribution to one of the thorniest causative problems in history—the origin of the two Italies. Anyone, however, who approaches this volume in the hope of finding a final answer will be disappointed. With wise reserve Abulafia avoids all temptations toward quick, simplistic answers while shedding precious light on the knowable facts that emerge from careful examination of the records. A regional or period specialist might find some minor sin of omission or variance in point of view, but overall Abulafia shows great care and excellent

judgment in the management of such vast material, and in marshaling the evidence so that it speaks for itself. It is to the great credit of this author that he manages to extract all that the documents can tell us but no more.

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CONSTANCE HEAD. *Imperial Twilight: The Palaiologos Dynasty and the Decline of Byzantium*. Chicago: Nelson-Hall. 1977. Pp. viii, 210. \$11.00.

Constance Head, a Byzantinist who published a biography of Justinian II (1972), has now turned to Byzantium's final age. After noting the lack of general surveys of the Palaiologan period (1259–1453), she writes, “. . . it is my hope that through this series of personal glimpses of the Palaiologan emperors and the time in which they lived, the intelligent layman or student of history will come to sense more vividly the nature of the long-vanished Byzantine world in its last tragic yet glorious years of imperial twilight” (p. 4).

Head concentrates on the personal lives of the members of the dynasty; what interests her are their characters, the vicissitudes of their political careers, their marriages, and their offspring. Insofar as the sources allow, she gives extended attention to stories concerning the various female members of the family: “Among the countless Marias, Theodoras, Helenas, and Irenes of the Palaiologan womenfolk, there is only one Simonis, a girl destined to a most unusual—and unfortunate—life” (p. 38).

While she uses the principal modern monographs, she writes directly from the sources, including such difficult, untranslated authors as Pachymeres. Her style conscientiously seeks vividness; her sentences are usually short and simply constructed.

The anecdotal approach sacrifices depth; few causes of Byzantium's collapse will be apparent to the reader. Because she focuses on the story of the dynasty, her account is limited to the highest aristocracy, and the reader will experience only one aspect of “the long-vanished Byzantine world.” This reviewer finds misleading the application of the term “nation” to the Byzantines (pp. 1, 30, 32, 169).

The book, which includes notes at the rear, an annotated bibliography, well-chosen illustrations, and a good index, can be recommended for high school and junior college libraries; the serious scholar will turn to such a work as D. M. Nicol, *The Last Centuries of Byzantium, 1261–1453* (1972).

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MODERN EUROPE

RICHARD KIECKHEFER. *European Witch Trials: Their Foundations in Popular and Learned Culture, 1300–1500*. Berkeley and Los Angeles: University of California Press. 1976. Pp. x, 181. \$13.50.

E. WILLIAM MONTER. *Witchcraft in France and Switzerland: The Borderlands during the Reformation*. Ithaca: Cornell University Press. 1976. Pp. 232. \$15.00.

The two books under review constitute noteworthy contributions to what E. William Monter has aptly termed “an ongoing renaissance in the historiography of European witchcraft” (p. 9). These are both well written, cogently argued works of painstaking research and careful scholarship.

The principal aim of Richard Kieckhefer's book is to sort out the respective contributions of popular and learned culture to the emergence of the European “witch-craze” in the sixteenth and seventeenth centuries. His analysis is based on a methodologically sophisticated and rigorously systematic assessment of the known late medieval judicial records (calendared in an appendix) from regions scattered throughout most of Western Europe. After a brief introduction the author traces through four basic stages the gradual evolution and elaboration of medieval witch trials, with particular emphasis on the charges that were leveled, where they were initiated (i.e., “from above” or “from below”), and the pattern and rates of prosecution. Kieckhefer argues that the merging of popular witchcraft with the science of demonology—a conjunction critical to the great witch-hunt—had occurred by the mid-fifteenth century. He shows clearly that, whereas the popular tradition had been (and would remain) thoroughly preoccupied with the threat of sorcery, or maleficent magic, the learned tradition, the culture of theologians and inquisitorial jurists, had come to focus on the notion of diabolism, the deliberate worship of the devil and his involvement in the act of sorcery. These demonological preconceptions, virtually absent from folk culture, were introduced into the trials through learned influence and superimposed on popular ideas of sorcery, thereby transforming the notion of sorcery itself and intensifying the fear and horror of witchcraft. As the obsession with diabolism became increasingly widespread and intense, charges against accused witches became correspondingly more serious, trials more frequent and sensational, convictions more likely, and punishments more severe.

Kieckhefer's analysis clearly takes us beyond the important recent studies of Russell and Cohn. A number of crucial questions remain unanswered, however. Kieckhefer tells us that he is interested in

showing how beliefs in witchcraft were “related to the tensions and anxieties of late medieval society” (p. 9). But to the extent that he does so, he treats only the popular notions, practically ignoring the learned ones. Nowhere does he satisfactorily explain why “a large and apparently growing section of the educated elite” responsible for conducting the trials should have suddenly become so preoccupied with—even obsessed by—diabolism in the early fifteenth century (and not before), or why the numbers of “educated people who did believe in diabolism . . . [should] have increased dramatically by 1500” (pp. 75, 80). We still do not know why popular and learned ideas of witchcraft came together when and where they did or what impact the medieval learned tradition had on post-medieval witch-hunting. Unfortunately, the picture we get of popular beliefs in and practices of witchcraft is an essentially static one; as Kieckhefer portrays things, for some two centuries virtually the only dynamic in the system was that supplied by the learned culture. He further claims that when jurists and theologians interjected the charges of diabolism they introduced “a new dimension to the craze that was already under way, adding fuel to an already blazing fire” (p. 105). Just on the evidence the author himself provides, however, it is hard to know what “craze” or “blazing fire” he has in mind. Likewise, given the sporadic incidence and irregular nature of major trials even as late as 1500, it is difficult to understand what he means when he speaks of the “full momentum” which the prosecution of witches supposedly attained during this period (pp. 10, 20). The contributions of the folk and learned traditions to the intensified concern with and increased prosecution of witches and the reasons for the conjunction of these traditions at this time are thus still in need of further analysis.

Though narrower than Kieckhefer’s in geographical scope and concerned with a somewhat broader series of issues, Monter’s book picks up essentially where Kieckhefer leaves off. In the early modern period the Jura region was a complex and confusing patchwork of states with a variety of religious, cultural, political, and legal traditions as well as strikingly different styles of witchcraft and patterns of witch-hunting. The heterogeneity of this area and the exceptionally rich and well-preserved character of its archives have permitted Monter to use these borderlands along the French-Swiss frontier as “a laboratory in which to test and compare ideas about the history of European witchcraft in ‘typical’ communities” (p. 8). Following a preliminary chapter which usefully traces the “rise and fall of witchcraft theory,” the author turns his attention to examining witchcraft belief and practice, official attitudes toward the phenom-

enon, the nature and “sociological dynamics” of accusations, and the incidence and results of trials. Like other recent scholars, Monter has found that the overwhelming majority of those accused and convicted of witchcraft came from among the weakest and most vulnerable members of society—the old, the poor, the handicapped, especially women. But many of his other findings regarding the Jura experience—the frequent incidence of trials in its rural villages, the highly irregular cycles of prosecution, the virtual absence of large-scale panics, the generally low rates of conviction and execution, and even the relatively large numbers of males accused of witchcraft in certain districts—contrast strikingly with the patterns seen in most other parts of Europe. Indeed, his overall findings indicate far more diversity in the nature of witchcraft and witch-hunting than has previously been observed and will undoubtedly force a further modification of the traditional picture of early modern witchcraft.

Unfortunately, however, while Monter does a good job of describing and analyzing the patterns of Jura belief and behavior and of relating them to the broader European context, he offers little explanation of what he has found. For example, though he addresses much of his attention to the impact which religious differences throughout the Jura had on attitudes toward witchcraft, he never makes clear why these differences should have been a factor in determining such matters as the relative harshness or leniency of treatment or the likelihood that children would be witchcraft suspects (pp. 105–07, 127, n. 18). (In this connection Monter fails to clarify whether and to what extent the demonstrable divergences between Protestant and Catholic demonological theory corresponded to disparities in Protestant as against Catholic popular practice.) Particularly frustrating is his discussion of demonic possession. We learn that “the seventeenth century was the golden age of the demoniac” (p. 60): a “special obsession” in France, demonic possession became in Geneva “the single most common *maleficium*”; and in Franche-Comté it was “built into the base of popular witchcraft” (pp. 10, 59, 72). We also learn that “these ubiquitous possessed persons were invariably . . . women and children” (p. 139). But nowhere are we told why any of these things should have been so. Are there any cultural, social, psychological, or other changes which might help to account for the sudden appearance of such possession phenomena, for the particular susceptibility of seventeenth-century women and children, or for the numerous witch trials held to deal with them? Was it that more people were now becoming “possessed” or that possession, which heretofore was “usually not taken very seriously,” was

occurring at much the same rate but had suddenly come to be viewed in a wholly different light? In either case, some attempt must be made to explain these dramatic new developments. There is also the need to reconcile the apparent inconsistency between Monter's assertion that judges in the Jura "refused to pay much attention to accusations (or even confessions) of witchcraft made by children" (p. 196) and what he says elsewhere about their "special role in the social drama of Jura witchcraft" (p. 127).

Clearly, much more work remains to be done before we are likely to reach any consensus regarding the explanation and interpretation of the great witch-hunt. Nevertheless, these two books considerably enrich the field of witchcraft studies and enable us to understand more fully the meaning and significance of witchcraft in the history of late medieval and early modern Europe.

B. ROBERT KREISER
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J. MICHAEL PHAYER. *Sexual Liberation and Religion in Nineteenth-Century Europe*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. 176. \$15.00.

The thesis of this book is that "in the first half of the nineteenth century . . . most Germans and Frenchmen, especially people in the lower class, became more sexually permissive while remaining religious" (p. 9). The end of widespread famines, the liberal reforms of the Napoleonic era, and the emergence of a wage-earning landless proletariat abolished the controls of family, community, and church that hitherto made premarital "sexual abstinence or restraint . . . a simple fact of life" (p. 23). After 1800 the rural proletariat no longer associated sexual behavior with religious morality and came to believe that neither God nor the state condemned bastardy. They spent their wages on stylish clothes and their leisure at new intimate dances. Proletarian children were raised in an eroticized, affectionate environment which guaranteed that they would be as sexually liberated as their parents. Paradoxically, these proletarian free lovers continued to attend church and even demanded that the numerous traditional holy days be observed, albeit for secular leisure purposes.

J. Michael Phayer believes that all these changes were accurately reflected in and also explain the rise of illegitimacy statistics of the German parish records he studies so minutely, as well as the many attacks on lower-class immorality produced by the clergy, bureaucrats, and other upright citizens in the *Vormärz*. Because he can think of no reason why clerical statisticians should begin recording more illegitimate births after 1800 than before, or

why they should falsify their records by attributing nearly all bastards to the rural proletariat, Phayer takes "the record at face value. After 1800 bastardy surged" (p. 42), proving that the poor had secularized their sexual behavior for the first time, and that a sexual revolution actually took place.

I would argue that the sexual revolution after 1800 was not the proletarian splitting of sexual behavior from religious morality that Phayer posits as modern, for he himself suggests that the compartmentalization of morality and religion by the poor "possibly . . . is conduct fairly representative of the premodern mentality" (p. 99). Rather it was the attempt of bourgeois moralists to fuse sexual morality and religious beliefs in order to control the behavior of a class they opposed, precisely because it still adhered to "premodern" sexual and economic mentalities. Nor am I convinced that "capitalism . . . abetted the sexual revolution in rural areas" (p. 35), by providing day-laborers and domestic workers with "pocket money" so that "they could be dashing in the latest cut of clothes and turn a pretty ankle" (p. 53). This was the complaint of bourgeois critics, but it does not jibe with the fact that rural proletarians limited their own earnings by clinging tenaciously to a folk calendar which forbade work on some 150 to 200 holy days. The same critics who attacked the poor for being promiscuous and spendthrift may have recorded more bastards after 1800 as proof of their charges. Landless rural wage-workers may have been the product of "modernization," but their sexual, religious, and economic values appear to have retained a distinctly premodern cast in the *Vormärz*.

ROBERT P. NEUMAN
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College at Fredonia*

RICHARD SCASE. *Social Democracy in Capitalist Society: Working-Class Politics in Britain and Sweden*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. 184. \$15.00.

All too often surveys of working-class attitudes are undertaken in isolation from any objective empirical examination of the actual system of class inequality that obtains in society. Richard Scase's book must be seen therefore as having the particular virtue of fusing an original comparative survey of Swedish and British manual workers' attitudes regarding inequalities of opportunity, economic condition, and power with a rigorous comparative examination of the actual dimensions of class inequality in the two societies.

Scase demonstrates that both societies remain

highly resistant to interclass mobility and continue to exhibit very wide disparities in income and wealth. Moreover, he shows that there has been remarkably little change along these dimensions over the last forty years. Scase attaches specific significance to the fact that, despite the Social Democratic Party having been in power over this whole period, Sweden is no more equal a society than Britain where the Labour Party has been in power for less than half the time. His conclusion is to the point: "The price of labor in Sweden continues to be overwhelmingly determined by the forces of capital accumulation" (p. 162).

But if the dimensions of class inequality are similar in Sweden and Britain, Scase's survey of two samples of manual workers in comparable engineering factories does indicate interesting differences in working-class attitudes. Swedish workers appear to be less conscious than the British of the closure of the class system and the narrow possibilities of mobility, but they are more aware of inequalities of economic condition between the classes and more resentful of them. Moreover, both the industrial and political wings of the labor movement have much greater legitimacy among Swedish workers, who tend to ascribe what gains they have achieved to the strength of working-class institutions, and who exhibit in general a more coherent and radical (although by no means revolutionary) working-class ideology.

Scase's explanation for these differences in attitudes is located at the level of the labor movements as agencies of socialization: i.e., the more egalitarian ideology of the Social Democratic Party and the central trade union federation (the LO) than of their British counterparts, the Labour Party and the Trades Union Congress. This provides a valuable perspective for comparative politics. It counters the thrust of much current sociological and public policy analysis which asserts that politics makes little difference to the evolution and operation of industrial societies; and it points to the contradictions contained within social democracy itself, as workers socialized by the rhetoric of "equality" and "socialism" develop beliefs which challenge the conservatizing class harmony orientation of social democratic governments.

Despite these merits, however, the book has some serious defects, the greatest of which is that Scase's main explanatory postulates are weakly grounded. He does not undertake a serious comparison of Swedish and British social democratic ideology, and hence his claims regarding the comparative socializing effect of marginal differences in ideology, while suggestive and plausible, are not convincing. Moreover, the tremendous emphasis he places on the importance of the institutionalized separation between white-collar and man-

ual workers' unions in Sweden as opposed to Britain is highly suspect.

Most regrettable of all, however, is the fact that Scase pays too little attention to the strengths and weaknesses of the dominant classes in the two societies and the comparative effects of their (different) value systems on the workers directly or on the social democratic parties themselves. Without an awareness of the cultural histories of the societies and particularly of the extent of hegemony of ruling-class ideas, his emphasis on ideology as an explanatory factor must be judged inadequate.

Finally, it must also be said that Scase consistently betrays a certain naiveté with regard to the effective representation of working-class interests in Sweden and the possibilities for fundamental change through the Social Democratic Party and the LO. He recognizes the role of social democracy in containing class struggle, but fails to incorporate this systematically in his approach. Why Swedish social democracy should transcend its historical tendency to incorporate working-class dissent within a "humane" capitalist system remains mysterious to the reader in the absence of an analysis of the economic contradictions facing Swedish capitalism and without consideration of the necessary conditions of political transformation. Greater attention to these sorts of questions would have turned what is a good book into an excellent one.

LEO PANITCH
Carleton University

M. R. D. FOOT. *Resistance: European Resistance to Nazism, 1940-1945*. New York: McGraw-Hill. 1977. Pp. xix, 346. \$15.00.

No portion of World War II history has been more favored by new light in recent years than that of clandestine operations. Evidence has been scanty or contradictory, controversy has been widespread and at times savage, and all too often claims have been extravagant. More than elsewhere, the release of official documents to the researcher has been hedged about with restrictions. The ebb of wartime passions and rising demands for public access to government data have done much to change this. Revelations on the Ultra Secret, Allied penetration of the mysteries of German code machines, have helped to erode some of the barriers.

One concomitant is a renaissance of resistance studies. It is curious how much Anglo-Saxon scholars have shied away from them in the past. During the war itself there was a tendency to underestimate the opposition to German occupation. Later, Cold War sentiments enhanced suspi-

cions of movements which were often closely associated with Communism and at times actually taken over by Communists. The present work of M. R. D. Foot may be described as the first good general survey in English, although translation of Henri Michel's *The Shadow War* in 1972 helped greatly to fill the gap.

Foot's credentials for writing the story are impressive. During the war he was British liaison to French resistance forces in Brittany, and he has taught for many years at Oxford and Manchester. His personal experience no doubt has provided him with significant insights. At the same time it has served to give a strong British emphasis to aspects of the study. There is some of this in the extravagant claim that it was Ambassador Sir Samuel Hoare who "succeeded in persuading Franco not to join Hitler" (p. 32). The outside (of continental Europe) focus is further demonstrated by heavy stress on operations involving the escape of Allied combatants, such as stranded flyers, mislaid paratroopers, and intelligence or special forces operatives. Foot is also fascinated by details of technical apparatus developed by the British and, to a lesser extent, by Americans: weapons, other lethal devices, and means of communication. Thus he often centers attention on trivialities in a work that claims to be "the most comprehensive ever written on the subject." In contrast to Henri Michel, Foot finds little to interest him in the indigenous roots of resistance.

The first portion of the book stresses common problems: what resistance was, who resisted, the forms taken, political factors, and the operational features that so much preoccupy the author. The men and women who risked their lives and fortunes, as well as the fate of those dearest to them, are described as the toughest elements in their various countries with "no inhibitions at all, and uncanny quickness of wit." Foot then proceeds to deal in a necessarily synoptic fashion with the resistance story in twenty-four European countries, also devoting a few brief pages to the other continents. Such a format perforce dictates frustrating generalizations. Less excusable is a dangerous and always questionable tendency to extend such generalization to judgments on national character.

The style of the book varies from spirited and crisp to heavy-handed. Rather astonishing is the lack of a prevailing sense of excitement in a volume that deals with so dramatic a subject. A more serious defect is the absence of an adequate analysis of the strategic significance or impact of the European resistance movements in the broader war scene. On the other hand, the book is tightly organized, which makes it a convenient reference for those seeking a quick appraisal of one or an-

other aspect of the resistance. We are left, however, with a need for a penetrating, above-the-battle, objective study covering the wider European resistance story.

HAROLD C. DEUTSCH
Army War College

ALFRED M. DE ZAYAS. *Nemesis at Potsdam: The Anglo-Americans and the Expulsion of the Germans—Background, Execution, Consequences*. Foreword by ROBERT MURPHY. Boston: Routledge and Kegan Paul. 1977. Pp. xxvii, 268. \$9.95.

At a time when respect for human rights is again a theme of American foreign policy, it is well to recall the dark period a generation ago, World War II and its aftermath, when millions of innocent civilians were deprived of life and liberty not only by the demands of war but also by the deliberate acts of governments. Alfred M. de Zayas believes that the expulsion of the German civilian population from areas of Eastern Europe at the end of the war was a horror and a tragedy to which the world has paid too little attention. He therefore set out to assemble all the data he could find, so that this case study would serve as a warning against future mass expulsions. In the course of doing so he wanders through and around the subject from several different angles.

The result is a somewhat spotty book, compounded of considerable research (competently done), interpretation (sometimes naive but generally not far off the mark), side excursions into various related but very large historical and political problems (necessarily incomplete and not particularly enlightening), and indignation (to which the author is entitled).

Was there any way for the Western powers to prevent the expulsions? De Zayas, although placing the major responsibility on those who insisted on pushing the Germans out (the Russians, the Poles, and the Czechs), makes much of the deficiencies of British and American policy. During the war they were pressed by Edvard Beneš to accept the proposition that the Sudeten Germans should be expelled from Czechoslovakia, and though they did not like it they finally agreed, subject to its being done under international auspices. At Potsdam they agreed too readily that the transfer of German populations "will have to be undertaken," contenting themselves with the saving clause that it should be done in an orderly and humane manner, which gave them legal grounds for appeals to the Russians but was in fact unenforceable. And although they never accepted the principle of collective guilt, in November 1945 they

agreed to very high numbers of Germans subject to transfer.

All this the author documents. But it does not quite substantiate his main conclusion that "it was undoubtedly Anglo-American adherence to the principle of population transfers that made the catastrophe of 1945-48 possible" (p. 184). Most of the Germans in the path of the Red Army were going to move or be moved no matter what the Western powers did. As for Poland, once Winston Churchill agreed at Tehran to the displacement of Polish geography from east to west, the migration of Poles from the lost eastern provinces and of Germans from Poland's new western provinces was the only logical consequence. Perhaps there was an alternative for Czechoslovakia. But, taking account of the trauma of 1938 for the Czechs, that alternative could hardly have been the author's proposition that it would have been better to let the Munich frontiers stand (p. 34).

There is much to be said for the principle that it is better to move frontiers than to move people. But the Germans had seized and ravaged territories in Eastern Europe, leaving millions of victims. There was bound to be some evening of the score. These were facts which inevitably blurred the moral issue of justice for the Germans. A letter of President Truman, which the author quotes and condemns as a reversion to "Morgenthauism" (p. 133), was perhaps callous but also understandable: "While we have no desire to be unduly cruel to Germany, I cannot feel any great sympathy for those who caused the death of so many human beings . . . I admit that there are, of course, many innocent people in Germany who had little to do with the Nazi terror. However, the administrative burden of trying to . . . treat them different from the rest is . . . almost insuperable" (pp. 133-34).

JOHN C. CAMPBELL
Council on Foreign Relations

NORMAN DAVIS, editor. *Paston Letters and Papers of the Fifteenth Century*. Parts 1 and 2. Oxford: Oxford University Press. 1971, 1976. Pp. lxxxvii, 671, i-xii; xxxiii, 664, xiii-xxiv. \$38.50; \$52.00.

A new and authoritative edition of the *Paston Letters and Papers* is a major event in the annals of fifteenth-century English historiography. The letters of the Paston family are the best, in both quantity and quality, of the various sets of private letters and papers that we have from the century. They are richer in personal and family content, more revealing of local society and politics, and more voluminous than are the papers of other families: the Stonors, making money from Cotswold and

Chiltern sheep; the Cely family, Merchants of the Staple; or the Plumpton, prosperous Yorkshire gentry. Amid the thin chronicles and the arid government records, they remain a rich, detailed, and very cranky addition from about 1450 to the mid-1480s.

The Pastons were a Norfolk family. They rose to prominence by virtue of the legal profession, through eternal wrangling over real estate and marriage, and by toadying favor with a great soldier of fortune of the French Wars, Sir John Fastolf. They served the great and themselves, and they preserved their correspondence. Because they were litigious and involved in innumerable affairs at all times, their papers are a prime source for the sort of local disorder and private warfare we characterize as "bastard feudalism." And in the midst of their concern for the main chance, they always found time to talk about the advancement of friends and relatives, the expense of sending children to the university, their health, and the need for clean laundry while travelling. The miscellaneous nature of the collection is a part of its charm.

Somehow the papers of the Pastons escaped the ravages of time and of man. They were first edited and published by Sir John Fenn in 1787. In a series of editions published between 1872 and 1904 James Gairdner reworked the materials to meet the demands of modern scholarship. Norman Davis is the first editor since Gairdner to undertake the task afresh, and we now have two-thirds of his appointed job. Volume 1 contains 421 letters and papers written by the Pastons. The materials are presented chronologically through the life of each of fifteen members of the family, so we have all the materials emanating from John I, then everything from John II, etc. Volume 2 contains 444 letters and papers, plus 63 related documents, arranged as in volume 1, only it is now letters *to* rather than letters *from*. The editorial apparatus treats the dating and the hand and provides references to the editions of Fenn and Gairdner. Volume 3 will treat the language, some more texts, and such scholarly devices as a comprehensive index. Would that it appear while university libraries still have budgets!

Davis has deliberately departed from the old principle of arrangement in strict chronological order without distinction either between members of the family or between outgoing and incoming correspondence. His plan makes it harder to follow a particular event or family theme. It requires use of the cross-references to correlate a letter received with the response it elicited, and for some issues one has to run through papers relating to a variety of Pastons. What do we get in return? There is the gain of being able to focus on one particular member of the clan and to trace his or her words

without interruption: we can see people change their style and their mode of expression over the years. The greater degree of internal organization has enabled Davis to correct the dating and placing of some two dozen letters. He is the first editor who has a special interest in the language and script of the letters. His great contribution—apart from the publication of some unedited texts and a good deal of editorial correction—has been to identify the scribal hands and to separate them from the Paston autographs. We can now speak with assurance about autographs and the use of scribes. That the women, for example, sent many letters does not necessarily mean that they themselves wrote many letters. To complement his remarks about language, and script, Davis includes twenty-four plates to reveal the variety of hands (as well as to awe us with his patience as a paleographer and cryptographer).

For the historian of fifteenth-century England this edition is a boon. It presents about seventy documents not in the main collections of Paston letters in the British Museum. If it offers few surprises, it is a handsome modern edition of a basic source. In our effort to make order of the past, it is nice to see that the same people who could write, "my lord of Clarans and the Dwek of Suthfolk . . . schold come down and syt on syche pepyll as be noysyd rytous in thys contre" (Letter 168, January 1462), could also say, "Ryght wurschypfull and welebelouyd Volentyne, in my moste umble wyse I recomande me vn-to yowe" (Letter 416, February 1477). We have long mined these letters. Through Norman Davis' great labor, we can all repay some of our debts to that long-winded family whose predilection for the written (and preserved) word has been so illuminating.

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N. F. BLAKE. *Caxton: England's First Publisher*. New York: Barnes and Noble. 1976. Pp. 220. \$22.50.

William Caxton was not England's greatest printer, nor did his mediocre translations and uninspired typography do much to promote the English Renaissance. But the middle-aged mercer from Kent who introduced printing to his native land in 1476 became the father of English publishing. N. F. Blake's volume, one of the numerous British contributions to the Caxton quincentenary, deserves a place alongside G. D. Painter's distinguished biography as a worthy commemoration of that event.

Some years ago Blake's *Caxton and his World* sur-

veyed the literary influence of Caxton's largely vernacular works. The present volume is intended as a companion study on the technical aspects of that first English press. A rather slight opening chapter which rehearses the shadowy history of early printing is redeemed by a fine account of Caxton's career from his early days as a London merchant apprentice through his advancement to the governorship of the so-called English Nation at Bruges. Caxton learned the new art of printing at Cologne and practiced it in Bruges under the patronage of the Yorkist Duchess Margaret of Burgundy. In the England of her brothers Edward IV and Richard III, and subsequently under Henry Tudor, Caxton made his real contribution in introducing the new technology. The principal part of Blake's book, however, is a thorough and often highly technical description of that first English press and its productions until the printer's death in 1491.

Unhappily, the book's format is ill-suited to its subject. The publisher, presumably hoping for a general audience, has produced an attractive volume without any footnotes and with only the briefest of bibliographies. Facts and speculations drawn from a variety of medieval sources and modern studies often go unattributed, and arguments necessarily summarized from the author's earlier works sometimes convey an idiosyncratic abruptness worthy of the opinionated Caxton himself. The chapters on Caxton's life as a merchant and publisher, on the role of aristocratic and mercantile patronage, and on the continuation of his press under Wynkyn de Worde suffer less from this absence of documentation than do the central sections on book production, typefaces and fonts, collation and gatherings, copyreading, and ornamentation. The ingenious extrapolations and speculative deductions which are needed to establish the techniques of early printing are not well served by the denial of a scholarly apparatus; even a recognized authority such as Blake cannot construct such elaborate technical arguments wholly without citations, without leaving his reader sometimes curious, occasionally puzzled, and often uneasy. We can hope that future editions of this much-needed study will be supplemented with appropriate documentation.

The volume itself is a welcome piece of book-making, with pleasant typography, good paper, generous margins, profuse illustrations, and a handsome and sturdy binding. Even the dust-jacket, however, betrays the ambivalence of author and publisher about its intended audience; the steel engraving of Caxton which adorns the back cover is the notorious portrait by Bagford which Blake rightly denounces in his text as spurious. It is characteristic of our knowledge about William

Caxton that we do not have the slightest idea how he looked.

J. W. MCKENNA
Haverford College

FELICITY HEAL and ROSEMARY O'DAY, editors.
Church and Society in England: Henry VIII to James I.
Hamden, Conn.: Archon Books. 1977. Pp. 206.
\$12.50.

This is a useful collection of eight original articles on a variety of subjects. As the editors note, they have made no attempt to impose unity of viewpoint. The articles do complement one another fairly well, however.

The best is probably Claire Cross' "Churchmen and the Royal Supremacy," a fine overall survey of the question in which the author establishes the very ambiguous attitudes which English ecclesiastics had toward royal power. Conflicts over matters of conscience were almost inevitable, especially with respect to the Puritans, and as time went on it became apparent that royal supremacy represented less a matter of lay control of the Church than the use of monarchical power to prevent such control, a process which climaxed in Charles I's support of Archbishop Laud.

Almost equally good is Rosemary O'Day's discussion of ecclesiastical patronage, an extremely tangled and often misleading field subject to frequent revision. She argues persuasively that, although patronage tended to come more and more into lay hands, its exercise was always fragmented and no group or class of persons was able to use patronage systematically to reshape the Church.

Felicity Heal goes over the often ploughed ground of the economic condition of the Church and shows that most prevalent generalizations are in need of significant qualification, although there is no doubt that the overall position of the Church declined because of a variety of factors.

Christopher Kitching's discussion of the hoary subject of the disposal of monastic lands is a useful corrective to established simplifications, and the article by Ralph Houlbrooke on bishops is also enlightening. Several other essays in the collection suffer from almost complete reliance on printed sources, random collection of evidence, and failure to generate very significant conclusions.

JAMES HITCHCOCK
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CLAIRE CROSS. *Church and People, 1450-1660: The Triumph of the Laity in the English Church.* (Fontana Library of English History, number 2.) Atlantic

Highlands, N.J.: Humanities Press. 1976. Pp. 272.
\$14.00.

The triumph of the laity in achieving eventual supremacy in the English Church forms the central theme for Claire Cross' interpretative essay. This volume, part of a series under the editorship of G. R. Elton, succeeds in meeting the goal of reinterpreting familiar and unfamiliar aspects of English history. Much of the book is devoted to a familiar recitative—enhanced by the author's integration of recent scholarship—on the progress of the Reformation in England. Acknowledging her indebtedness to A. G. Dickens, Cross augments her text with examples from contemporary sources which emphasize the importance of the laity in determining the religion of the nation.

In many cases specific interpretative notes sounded by Cross are worth re-emphasizing: the significance of Lollardy as one aspect of extensive lay questioning of the late medieval Church, the often overlooked improvements effected by Church authorities between 1460 and 1520, the virtual emancipation of the laity with the circulation of vernacular Bibles, the impact of Mary Tudor's reign in determining "the subsequent development of the English Church" (p. 124), the consolidation of Protestantism during the reign of James I, and the unmitigated long-range failure of the Laudians to win substantial lay support. Throughout, Cross underscores the diverse and at times contradictory range of lay belief and practice, aptly charting, for example, varieties of Catholic survivalism, Protestant radicalism, and Separatism. Cross also gives overdue attention to the active participation of laywomen in both Church and sect.

Cross' chronicling of events seems less sure-footed in the mid-Tudor period. Her discussion of Henry VIII's attitude toward the laity would have benefited from L. B. Smith's insights (*Henry VIII, The Mask of Royalty* [1971]); and her evaluation of the Protestant polity under Edward VI, and in particular the rebellions of 1549, ignores the contributions of W. K. Jordan (*Edward VI*, 2 vols. [1968-70]). One wonders why these rich resources from American scholars are neglected.

Cross' central thesis of the laity's increasing insistence on religious freedom falters in the final chapter of this book. She has difficulty accounting for the period of active persecution of Non-conformity in the Restoration which "derived its impetus largely from this sector (i.e., the parliamentary) of the laity" (p. 233), and which followed the long struggle for religious pluralism of the Interregnum. Her designation of the Restoration as an "Epilogue" does protect the general validity of her thesis for the years between 1450 and 1660, yet the goal of religious toleration was, with no-

table exceptions, only dimly grasped by clergy and laity alike in the sixteenth and seventeenth centuries.

FREDRICA HARRIS THOMPSETT
Seabury-Western Theological Seminary

FRANK FREEMAN FOSTER. *The Politics of Stability: A Portrait of the Rulers in Elizabethan London*. (Royal Historical Society Studies in History, number 1.) London: The Society. 1977. Pp. x, 209. \$12.00.

Students of London's history should welcome this slim volume as a much-needed contribution to an important but neglected subject. Although the literature on London's pre-Civil War history is rich in edited chronicles, popular histories, and legal treatises, there is a dearth of sophisticated explanation and careful analysis, particularly for the Tudor period. Building on the work of Alfred B. Beaven and making imaginative use of the sketchy *Journals* and *Repertories*, the author has fashioned a work in the tradition of the excellent studies of medieval London by Sylvia L. Thrupp and Gwyn A. Williams.

As accurately indicated by the title, Frank Freeman Foster's major theme is the extraordinary political stability enjoyed by London's government during an era that is generally characterized as being dynamic. He argues that the key to this stability can be found in the nature of London's rulers. Going beneath the outward form of the institutions of civic government, Foster examines the many interrelated facets of the ruling oligarchy's lives to demonstrate that participation in City politics was not only inseparable from other aspects of their lives, but also the primary factor in shaping their world-view. In eight substantive chapters, Foster re-creates the social, political, and psychological environment of the aldermen and common councilmen who wielded almost absolute power in the twenty-six wards of the City. His examination of elections, offices, procedures, levels of authority, and ranks within the ruling group are fleshed out by revealing inquiries into their participation in trade, religion, economic and familial connections, and the relationships between social and political standing. These investigations disclose a powerful traditionalism and a strong sense of community that made political participation the keystone of the rulers' lives and determined "how wealthy they became, whom they married, even how they felt about such things as religion, the poor, the queen, and their own rulership" (p. 11). Since the attitudes and behavior of the rulers were constant and their power substantial, they were able to resist disruptive change and maintain a desired stability.

Several aspects of the book deserve special notice. On the negative side is the author's classification of rulers into three groups according to type and number of political positions held. No reason is given for this arbitrary division into "elite," "notables," and "leaders." This scheme is not really helpful in understanding the ruling group; in fact it is often confusing. In addition, it does not appear that such rigid distinctions were made on this basis at the time. On the positive side, and far outweighing this unnecessary categorization, is Foster's careful and imaginative reconstruction of the attitudes, beliefs, and assumptions of those who ruled the city. Although there are very few explicit comments by the rulers on their notions of politics and society, Foster demonstrates how the agendas, procedures, and the form and length of records reflect attitudes on efficiency, competence, duty, and loyalty and thus provide indirect testimony to the rulers' purposes. Scholars should also find useful the lengthy appendixes that include lists of the rulers and civic offices. Although a work of intentionally narrow focus, this book is a valuable addition to our knowledge of Tudor London.

ROBERT J. SMITH
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ALAN G. R. SMITH. *Servant of the Cecils: The Life of Sir Michael Hickes, 1543-1612*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. 220. \$15.00.

The Elizabethan political scene, particularly the patronage system, continues to hold great interest for Tudor and Stuart historians. There Court and Country came together in a symbiotic relationship in which both accommodation and strain were quickly reflected. A major figure in the late Elizabethan patronage machine was Sir Michael Hickes, secretary to William, Lord Burghley, and friend to his son, Robert Cecil. Alan G. R. Smith has mined the massive vein of Landsdowne manuscripts for an intriguing biography of Hickes as patronage secretary, moneylender, sometime Puritan, and local official.

Son of a London mercer and brother of the Jacobean financier Baptist Hickes, Michael attended Trinity College, Cambridge and Lincoln's Inn, where he made the contacts who provided his entrée into Burghley's service. The close relationships he formed then with the Puritans Thomas Cartwright and John Stubbes continued even after he entered government service in 1573. But from the 1590s on, Hickes' Puritanism waned and he devoted himself to his primary interests: patronage and profit.

Wallace MacCaffrey's analysis of Elizabethan

patronage finds illustration in Hickes' career. The Elizabethan elite, two to three thousand strong, angled strenuously for the rewards in the Crown's gift, and at times Hickes handled sixty to one hundred petitions a day. Of his correspondents, one third were peers, the rest gentlemen from every part of the country. Although some suitors already had the queen's approval, they still needed Burghley's—and Hickes'. Graft was, as Joel Hurstfield has argued, an integral part of the system: in the majority of suits Hickes apparently received promises of payment. He himself referred jokingly to "us poor bribers at court" (p. 68); and one suitor, having heard that Hickes was honest, offered him £60 for his services. Finally, when an ecclesiastic asked how to secure the secretary's aid, Hickes' nephew responded, "he was as well to pay as to pray" (p. 144).

Hickes used his gains from office, in his ventures as moneylender and provided an important link between courtiers and the money market. His clients included several peers, Francis Bacon, and Sir Walter Raleigh. His mother, a fascinating character, was herself a moneylender to the aristocracy, and did not hesitate to call down the vengeance of a just God on a dilatory debtor. The reaction was usually meek submission. In a strongly hierarchical society, the Hickes family was on familiar terms with the greatest men in the realm, some of whom were their debtors.

At Burghley's death in 1598, Hickes left the court and until his own death in 1612 held local offices as feodary, receiver-general of crown lands, steward of royal manors in Essex, deputy in the alienation office, and justice of the peace. In these positions Hickes carried out Robert Cecil's systematic program of increasing royal revenues at all levels of administration. Parlaying his earnings from office, moneylending, and marriage to a rich widow, Hickes bought land and, in characteristic English fashion, set himself up as a country gentleman; his descendants are the earls St. Aldwyn.

Clearly written and well researched, Smith's biography sets Hickes' career in the framework of Elizabethan institutions with careful descriptions of the workings of wardship, the money market, and local administration. The author does leave some larger questions unexplored, particularly the ways in which the patronage system shaped as well as responded to social, economic, and political change. For instance, was it Essex who distorted the patronage system as traditionally asserted? Or was it rather the pressure of a growing elite on decreasing royal resources, so characteristic of Jacobean patronage, which was already at work in the 1590s? Nonetheless, this lively biography provides important detail about the operations of the late Elizabethan court, especially the

mutually beneficial connections of aristocrats and merchants in Elizabethan patronage networks.

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WILLIAM S. POWELL. *John Pory, 1572–1636: The Life and Letters of a Man of Many Parts*. Microfiche Supplement, *Letters and Other Minor Writings*. Chapel Hill: University of North Carolina Press. 1977. Pp. xviii, 187. \$16.95.

John Pory is known to history chiefly as the translator of an early work on Africa and for his service both with and against the Virginia Company. He was also a member of Parliament, wrote at least two other published works, served his government and the Levant Company overseas, and became in his later years a prolific writer of newsletters. Since 1952 William S. Powell has pursued Pory through the published works of historians and the manuscript collections of England and America. The result is this short but interesting biography and a collection of more than three hundred fifty microfiche pages of Pory's letters and minor writings made the more serviceable by excellent notes and a thorough index.

The biography consists of three chapters, the first on Pory's career in England and on the Continent, the other two on his connection with Virginia. It is these latter two chapters which are Powell's main contribution. They deal in detail with Pory's role in the establishment of the Virginia assembly, his explorations in the colony, and his service on the Harvey commission; and they dispute the view common among historians that Pory was a tool of the Company faction headed by Robert Rich, second Earl of Warwick.

The first chapter is less satisfying. While the external events of Pory's English and Continental careers are adequately set forth, no more than a modest attempt is made to analyze his writings. We never discover the mind of John Pory or how he stood on the important issues of the day, particularly during his period as newsletter writer between 1625 and 1633, when the burning questions were the power of Charles I at home and the progress of the Thirty Years War abroad. For this we must turn to the microfiche letters. Pory's views emerge best in his heroes, of whom the greatest was Gustavus Adolphus. There were a host of lesser ones, including Sir John Eliot and Frederick V of Bohemia. It is perhaps significant that Pory's career as newsletter writer came to an end shortly after December 1, 1632, when one of his newsletters reported the death of all three.

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LELAND J. BELLOT. *William Knox: The Life and Thought of an Eighteenth-Century Imperialist*. Austin: University of Texas Press. 1977. Pp. xii, 264. \$12.95.

William Knox was an undersecretary in the American Department and a prolific pamphleteer. His writings, usually published at government expense, provided rational defenses of the imperial power's policies for the colonies. In preparing this lucid biography, Leland J. Bellot has successfully mined the manuscript sources (especially the Knox Papers at the Clements Library). He has also thoroughly familiarized himself with the relevant secondary materials, including some rather obscure works. In fact, unless a totally unexpected cache of manuscripts comes to light in the near future, it is highly unlikely that anyone will ever have to write a biography of William Knox again.

The author has interwoven two basic themes throughout this book: Knox's life and his thoughts about colonial policy. In the former instance, his Scots-Irish family background is described as a potent influence, although it did not prevent him from entering the Anglican Ascendency, nor apparently, from attending Trinity College, Dublin. The author is to be congratulated for resisting the psychohistorian's temptation to speculate about the significance of the miniscule part played by Knox's mother in his memoirs. Like Edmund Burke and others of his countrymen, Knox soon traveled to London, seeking a place, if not a fortune. The best appointment he could obtain was that of Provost Marshall of Georgia. While fulfilling those duties he acquired both land and slaves in the Savannah Valley, and he soon returned to London as agent for the colony. His return to the capital coincided with the need for "experts" to advise on problems connected with the Peace of 1763. He quickly became acquainted with Lord Hillsborough, the first secretary of state for the colonies, and served him and his two successors (Dartmouth and Germaine) as undersecretary. Ironically, Germaine did not trust Knox with military affairs; thus, when the disaster of Saratoga struck, Knox was one of the few highly placed officials who could claim complete innocence and total lack of involvement. After the reform movement which dissolved the American Department, Knox devoted most of his time to his family, his estate, and valiant efforts to recover some of his losses as a loyalist.

It is difficult to sympathize with Knox's imperialist ideas, although his convictions were always sincere and his reasoning very logical. He believed in strong monarchy and was convinced that most of the colonies already had "too much" democracy (which led to riots and demagoguery). As an ardent mercantilist he not only advised Lord Shel-

borne to confine the settlers to the territory near the Atlantic Coast (with the famous Proclamation Line of 1763), but he also opposed the later offer of peace with free trade to the newly independent states. His evangelical Anglicanism led him to view slavery as a benign institution in which the master had an obligation to introduce Christianity to the Africans. Knox's ideas were not original, but their significance lies in the fact that they were so widely held, by George III and many other officials. Reading these old-fashioned theories of empire helps to explain why so many "reasonable" men could lose thirteen colonies.

In sum, this is a first-rate biography of a second-rate man.

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JOHN P. CLARK. *The Philosophical Anarchism of William Godwin*. Princeton: Princeton University Press. 1977. Pp. ix, 343. \$16.50.

This volume of exposition and analysis seeks to rehabilitate the ideas of William Godwin, whose philosophy for a brief period in the 1790s captured the imagination of young English radicals. Godwin's faith in the redeeming powers of human reason, so inspiring in 1793 when *Political Justice* was first published, rapidly became anathema as the decade progressed. By the time Malthus composed his devastating polemic in 1798, Godwin was already a spent force, destined to live out his remaining years of obscurity by revising and expanding upon his forgotten philosophy. John P. Clark wishes to rescue him from this neglect because he believes that "Godwin should be considered a social and political thinker who is in many ways the equal of major figures like Locke, Burke, and Mill."

Clark divides his study into three parts. First, he examines Godwin's underlying principles, showing among other things that his determinism was not incompatible with the individual expression of moral approval and disapproval. Godwin was an egalitarian and extreme individualist but he was not, Clark maintains, a naive optimist. Second, Clark plants Godwin's ethics in the familiar ground of utilitarian moral philosophy. Like many of his predecessors, Godwin believed that pleasure was the only intrinsic good, and he evolved a theory of rights and duties which reflected this conviction. Finally, Clark devotes over half his book to Godwin's political and social theories. Godwin's distaste for governmental authority and his eloquent attack on private property derived from his radical utilitarianism, but he was not a political

revolutionary. Instead, his hope for the future lay in education and the free exchange of ideas.

Throughout his study, Clark carefully distinguishes his own views from those of other recent commentators. Some of these disputes are illuminating, while others are little more than quibbles masquerading as refutations. Moreover, Clark's weak grasp of historical context leads him to overestimate the originality of Godwin's ideas and to underestimate the personal and political factors which often account for their bewildering inconsistencies. As a result, this volume, written by a teacher of philosophy, may well disappoint the historian. Nevertheless, it is a clearly organized and useful examination of one of the more interesting thinkers of the late eighteenth century.

D. L. LEMAHIEU
Lake Forest College

D. L. LEMAHIEU. *The Mind of William Paley: A Philosopher and His Age*. Lincoln: University of Nebraska Press. 1976. Pp. xi, 215. \$12.95.

D. L. LeMahieu wisely emulates William Paley. Although his is not the "homey, racy, vernacular English" attributed by De Quincey to Paley, it is utterly clear and pleasing throughout. His thoughts are directed to the specialist and non-specialist reader alike, with respect for both. All in all, this handsomely produced book contains much good sense on the character of Georgian religious thinking.

LeMahieu's argument is forthright: Paley is synonymous with the English Enlightenment which constitutes an "age." Its distinguishing characteristic is a belief in the Deity and His beneficence. But how best to establish this in a scientific period? By using the tandem philosophical method of the argument from design and reasoning by analogy from the seen to the unseen. The result is a natural religion which satisfactorily meets the challenge offered by a particular form of science. Thus baldly put, LeMahieu's discussion seems familiar enough, but he presents it with a particularly fine flourish by suggesting that large numbers of thinkers from Shaftesbury in the seventeenth century to Whewell in the nineteenth accepted the primacy of a teleological approach. It was Darwin who untied the ribbon which held the package together. Paley's conjuring trick was exposed and the world-view broken.

The choice of Paley to illustrate *telos* in the Enlightenment is absolutely right. LeMahieu's position is to be respected and believed, but his argument, like Paley's, is achieved with too much ease. The book's subtitle points to the problem. An "age," even the eighteenth century, is more than a

consensus. It includes tensions that arise from the actual conflicts of everyday life and inevitably find their way into theology and philosophy. Because he is a social animal, man is perforce a contradictory one; and clear thinking, like Paley's, achieves its lovely economy at some personal cost. When the going is hard for Paley, or just when he is nudging some crucial ambivalence, LeMahieu paradoxically assists him. For example, we are never given an extended analysis of eighteenth-century views on eternal damnation, the foundation of Paley's entire system of utilitarian ethics. Also, although we are correctly told on several occasions that Paley's friendly intelligence must not be considered void of emotion, we are still left guessing how we as historians can appreciate the spiritual authenticity of Paley's religion if no personal struggle seems to have been involved.

There are other places where a too-brief discussion leaves the reader unsatisfied. Since we are given a morsel instead of a banquet on the difference between the English and Continental Enlightenments, hunger may perversely lead us to the thought that the union of science and religion in England owed more to the Renaissance than to the Age of Reason. Paley's political trimming is not cleanly handled. LeMahieu calls him an apologist, suggests that he can sometimes also be read as a proto-liberal, and casually associates him with the notion of a free marketplace of ideas. Paley's strictures on Mammon are puffed up into a democratic ideology when they are perhaps better seen as commonplaces from a period in which the maldistribution of wealth was a special social problem for the clergy. Other factual references could have been pressed into service. The Bangorian controversy would have aided the comparison of Paley with Warburton.

A short book cannot cover all topics, but if the history of ideas is to be used to pry a culture open then the reviewer has an obligation to question an approach that is for the most part short on historical reference, omits some of the interesting institutional factors that account for belief, and skirts the issue of how a value consensus arises in response to deeply-experienced challenges. Hopefully LeMahieu will elaborate upon some of his hints later on.

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DAVID CRESAP MOORE. *The Politics of Deference: A Study of the Mid-nineteenth Century English Political System*. New York: Barnes and Noble. 1976. Pp. 529. \$19.50.

NORMAN ST JOHN-STEVAS, editor. *The Collected Works of Walter Bagehot*. Volumes 5–8, *The Political Essays*. London: The Economist. 1974. Pp. 449; 407; 455; 451. £25.00 the set.

Coming together for review here are part of a new edition of Bagehot's writings and a scholarly monograph that has much to say about a subject he invented—deferential politics.

These four volumes of Bagehot's work contain his well-known treatises *The English Constitution* and *Physics and Politics* as well as shorter and more topical essays. Although some treat French and American politics, most are on English politics, and all are dug out of those weekly and monthly journals, chiefly the *Economist*, to which Bagehot was a prolific contributor. With these volumes, Bagehot's collected works, under the editorship of Norman St John-Stevas, now reach volume eight. Originally planned for eight volumes, the edition is now expected to reach twelve—two of literary essays, two historical, four political, three economic, and a final one of letters, miscellanea, and bibliography. The volumes of literary and historical essays were introduced by short appreciations of Bagehot as literary critic and historian. The political essays are prefaced by a much longer appreciation, written by St John-Stevas, of Bagehot as a political thinker.

Like the first four volumes, the present batch is splendidly edited—in every way an improvement on the edition by Bagehot's sister-in-law, Mrs. Barrington. St John-Stevas has grouped Bagehot's writings by subject—which is more useful than Mrs. Barrington's chronological arrangement. He has included a great deal more of Bagehot's weekly editorializing in the *Economist*—which is all to the good. And he has provided elaborate explanatory notes, an exhaustive seventy-eight-page general index, and even an index of Bagehot's epigrams—which one can be sure will be well quarried.

St John-Stevas' commentary on Bagehot's politics, however, is less satisfactory. In a previous issue of this journal (June 1976), I noted that history had been kind to Bagehot's reputation. I did not mean by this to deny Bagehot his virtues—but merely to suggest that they have been excessively commended and that it is time to inject a note of criticism. Although some political scientists and historians have begun to do so, St John-Stevas is not among them. He heaps praise, for example, on *Physics and Politics*—in form and content surely one of Bagehot's inferior works. He swallows whole, moreover, Bagehot on deference, and he neglects to point out how hard Bagehot found it both to be consistent about his idea of deference and to make that idea a useful tool of analysis. One suspects that what St John-Stevas finds most attractive in

the idea of deference is its congeniality for certain brands of twentieth-century English conservatism. He is, after all, a practicing Conservative politician, of a distinctly intellectual cast of mind.

On the other hand, D. C. Moore puts himself firmly in the company of Bagehot's new critics, being especially critical of Bagehot's idea of deference, which he dismisses as "patent nonsense" (p. 12). As Moore explains Bagehot's meaning, deference (or "influence," which he seems to use interchangeably) "obtained because a sufficient proportion of the electorate was composed of men of at least middling status, who were thus sufficiently *intelligent* to recognise the greater *intelligence* of their social superiors" (p. 12), and thus to vote for them. Moore will have no part of this. To explain voters' behavior, he argues, one should go to the poll books—those registers of electoral preference which flourished until the Ballot Act of 1872—and to the study of which Moore has devoted much time. These, he contends, show nothing about voters' intelligence; instead they show a great deal about "those hierarchically structured communities or networks" to which voters belonged and which ultimately account for how they voted. The mistake that Bagehot and others made was to substitute "an intellectual for a social dynamic" (p. 13).

It is this social dynamic, manifesting itself in "hierarchically structured communities or networks," sometimes described as "deference communities," which provides the key to Moore's provocative book. This is a work of much industry and of a sharp (if somber) intelligence, but made needlessly difficult by an unhappy literary style. The book falls into two main parts. The first deals with case studies of "deference communities": Huntingdonshire, Cambridgeshire, Northamptonshire, and North Northamptonshire. These were chosen, as Moore explains, because rural counties, dominated by great landowners presiding over dependent tenant farmers, produced the most revealing poll books—showing more clearly than other sorts the working of those deferential electoral blocs made up of farmers and landowners' dependents generally. Blocs of a similar sort, he suggests, probably operated elsewhere than in rural counties (pp. 6, 15), but less visibly so in their respective poll books.

The second part of Moore's book deals not with the machinery and incidence of electoral blocs but with their place in the political strategies of party leaders, Whig and Tory. Thus the first and second Reform Acts, according to Moore, were not intended to redistribute political power but were instead meant "to increase the strength of the traditional social nexus by defining and redefining the various constituencies in terms of the tradi-

tional groups and communities" (p. 430). When this kind of legislative effort proved impossible, then it was all up with traditional politics in England.

An adequate discussion of Moore's principal theses needs more space than this review gives them. But one general point concerning Moore's approach to his subject might be made. Like many historians he abhors human muddles, and perhaps more than most he insists on tidying them up. But there is a great deal of irremovable muddle in human history—in men's view of things, in why and how they do what they do. There was more muddle in Bagehot's idea of deference, in party strategy over Reform bills, and in the working of deference communities than Moore seems willing to admit. Bagehot often entertained an idea of deference (as in *Physics and Politics*) that had little to do with intelligence. Grey and Disraeli had more in their minds than merely buttressing the traditional nexus. And the machinery of deference in rural counties—as Davis' account of Buckinghamshire and Olney's of Lincolnshire portray it—was neither so prevalent nor so automatic and simple as he seems to make it. There was in these counties, to use T. B. Nossiter's language, a politics of opinion (or intelligence) as well as a politics of influence (or deference).

In short, Moore's book would seem to have substituted a new simplicity for an old. On the other hand, it does a great deal—together with a body of important articles on which it is in part based—to prompt everyone concerned to take a fresh and more searching look at English electoral politics. Political scientists have recently assured us (*American Political Science Review*, 68 [1974]) that the "simple act of voting" (as the article is entitled) is far from being simple, even for the most accomplished of modern psephologists. The harder the historians look, then, the better.

DAVID SPRING

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OLIVER MACDONAGH. *Early Victorian Government, 1830–1870*. New York: Holmes and Meier. 1977. Pp. 242. \$16.50.

His title notwithstanding, Oliver MacDonagh's new book is not really a study of early Victorian government, however that elusive term might be defined. Rather, it is an extended version of his controversial essay, "The Nineteenth-Century Revolution in Government: A Reappraisal" (*Historical Journal*, 1 [1958]: 52–67). But, in a work addressed to "the sober first- or second-year undergraduate," he has abjured the intoxications of controversy. He promises to return to these later. In the meantime he has reiterated, clarified, and—

possibly—somewhat modified his argument concerning "the revolution in social administration in the middle quarters of the nineteenth century which transformed a loose old-fashioned polity with few central functions and little central power into a much more actively and nationally regulated society" (p. 1). And, in obvious response to Henry Parris' challenge ("The Nineteenth-Century Revolution in Government: A Reappraisal," *Historical Journal*, 3 [1960]: 17–37), he has tried to show that his model fits far more than the facts of emigrant regulation. Thus, after devoting his first chapter to what he calls "The Favouring and Resisting Forces," he discusses how his "revolution," somewhat anticipated in Ireland, was manifest in England in factory regulation, coal mine legislation, poor law and local government reform, public health and sanitation, the establishment of a national police force, and the reorganization of the civil service and its abstraction from patronage politics. Except in the chapters on coal mining and Ireland, where he draws upon his own contribution to the Kitson Clark *festschrift* ("Coal Mines Regulation: The First Decade, 1842–1852," in *Ideas and Institutions of Victorian Britain*, ed. R. Robson [1967], 58–86) and upon his own recent survey of modern Irish history (*Ireland* [1968]), his evidence derives from the standard secondary literature.

MacDonagh has given his "sober . . . undergraduate" much to think about—although less than if he had also asked him to consider the intricacies of the five-step model of "revolution in government" described in his controversial essay. And very much less than if his argument did not involve a fairly simple polarization of "forces" and did not allow him to focus his attention upon the central government. Indeed, it scarcely exaggerates the case to say that the only events outside of Whitehall and Westminster—and, possibly, Clapham and Queen Square—which have any real importance to his "revolution" were those which created the conditions which occasioned it. The "revolution" occurred when, as an almost automatic response to the "intolerable" conditions produced by population growth, urbanization, and industrialization, humanitarianism generated an inspectorial bureaucracy whose functions, powers, and personnel were subsequently multiplied. "By a relatively early stage in the nineteenth century," he declares, "humanitarian sentiment prevailed in public attitudes whatever private conduct or personal motivation may have been; and when the 'climate of opinion' was predominantly humanitarian, when that opinion was reflected even obliquely in parliament and legislation and when the true circumstances of many branches of society were laid bare, the road was open for a transfor-

mation in the nature of the state" (p. 7). Arguing that Benthamism was more important for its method than its content, he focuses upon Chadwick, Ashley, and various officer-veterans of the Napoleonic wars who, as he observes, "supplied a high proportion of the new officials of 1815-40" (p. 5); upon the evidence gathered by select committees and royal commissions which, he claims, "enabled the administration to act with a confidence, a perspective and a breadth of vision which had never hitherto existed" (p. 6)—and this even though the evidence was often conflicting; and upon the provisions of different bills. In effect, while his "revolution" was achieved by legislation, it was only political, if the dawn be political.

One hears the voice of G. M. Young. But the problem was really set by A. V. Dicey, to whom MacDonagh acknowledges a large debt. Clearly, what MacDonagh received from Dicey were not only the "crude, old-fashioned but still indispensable stereotypes of old toryism, middle class liberalism and working class radicalism" (p. 12), but also the assumption that law is more the product of formal "opinion" than a means of regulating social relations in a given context; and he shares Dicey's notion that "individualism" existed in the mid-nineteenth century more as a social reality than as a rhetorical defense of the roles of the different elites in the many formal and informal communities of the kingdom. If there really was "an age of . . . positive and aggressive individualism" when the foundations of the modern collectivist state were laid (p. 9), if conceptual architects are generally necessary, and if no such can be found, then perhaps "humanitarianism" provides a way out of the consequent difficulty. But only if it be adequately defined—in effect, only if the question be answered: what were the trade-offs?

A review is not the place to keep score among MacDonagh and, *inter alia*, Henry Parris, Jenifer Hart, and A. J. Brundage. Presumably, some of MacDonagh's earlier critics will still be skeptical—in particular those who have considered the general problem from local perspectives and those who are interested in the structural role of values. But, while awaiting MacDonagh's fuller and, one hopes, more analytical statement of his case, they will join the others in their gratitude to him for having done so much to formulate the question.

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JOHN WOODWARD and DAVID RICHARDS, editors.
Health Care and Popular Medicine in Nineteenth Century England: Essays in the Social History of Medicine. New York: Holmes and Meier. 1977. Pp. 195. \$18.00.

JEAN DONNISON. *Midwives and Medical Men: A History of Inter-Professional Rivalries and Women's Rights*. New York: Schocken Books. 1977. Pp. vi, 250. \$14.95.

Until recently, the history of British medicine has largely been the domain of two rather disparate groups—historians of science concerned with developments in medical knowledge and techniques, and doctors interested in studying their professional forebears and the progress of their craft. The foundation of the Society for the Social History of Medicine in London in 1970 symbolized a change, then already under way, in the constituency and interests of those pursuing the history of medicine. A social—or sociological—approach to the history of medicine offers the possibility that the study of sickness, health, the treatment of disease, ideas about the human body, and the men and women who participated in the delivery of health care may have value for historians with a more general interest in social structure, social policy, and the history of ideas.

In varying degrees the two works reviewed here illustrate developments in the social history of medicine in Britain which bring narrowly medical issues into the wider arena of the history of British society. John Woodward and David Richards have assembled a collection of essays on a variety of socio-medical themes. The editors' extended introductory essay offers an excellent survey of accomplishments in the social history of medicine to date. It also attempts to delineate the relationship between history, medicine, and the social sciences, and to point out areas where further research is needed. Janet Blackman explores popular theories of generation in her analysis of *Aristotle's Works* (a handbook of medical lore for laymen), while Angus McLaren and Jean L'Esperance treat birth control and Victorian medical notions of sex roles in their respective essays. Perhaps the most useful essays are those by Ian Inkster and Ivan Waddington. Both of these authors take a more systematic sociological approach to medical issues, Inkster in treating the problem of social marginality among Sheffield medical practitioners and Waddington in surveying the conflicts between consultants and general practitioners in the decades preceding the Medical Reform Act of 1858. Predictably, the essays vary in quality. Some suffer from poor organization and wordiness, while others are too brief to develop argument and documentation as fully as they might.

The other volume here reviewed, Jean Donnison's study of midwifery, surveys obstetric practice by women from the Middle Ages to the present, but the central focus of her work is the nineteenth

century. Midwives, threatened with extinction in the Victorian years because of the growth of the medical profession and doctors' encroachment on this distinctly female sphere of health-care activity, sought to improve their occupational and economic position by lobbying for state registration, much as doctors, dentists, and other professions had done before them. Although the title suggests that Donnison treats women's rights extensively in her work, she is much more concerned with issues of social policy and administration—not surprising in view of the author's background and training. *Midwives and Medical Men* is replete with information on the public and parliamentary debates over the role of central government, local authorities, and the medical profession in the licensing and control of midwives. The author traces in detail the progress of legislation for the registration of midwives from the early nineteenth century to final adoption of the Midwives Act in 1902.

At times Donnison links medical opposition to the cause of midwives to doctors' antifeminism and fear of economic competition; at other times she suggests (and I think rightly) that medical men opposed midwives' advancement because of the larger question of their status insecurity and their unwillingness to be socially tainted by too close an association between medicine and the "inferior" activities of these unqualified but often able women. She pursues neither of these themes as systematically as she does the legislative and social policy aspects of her topic. And readers wishing to see any extended treatment of the relationship between midwifery and the larger issues of women's occupational opportunities, feminism, and women's rights may be disappointed by the brevity of her discussion of these issues. However, Donnison does provide readers with a fascinating view of how women tried to use the tactics of professionalization to improve their occupational and economic circumstances. And she uncovers, in her exploration of the midwives' struggle for improved status, not only their battles with the male medical profession but also their struggles against opposition from women who were seeking to advance the cause of nursing in the late nineteenth century.

Both these volumes, in the process of addressing issues in the social history of medicine, open windows on larger issues of Victorian history. They show how medical history in a social context can be intimately tied to questions of social structure, social mobility, professionalization, the history of women, social welfare, sexuality, and related issues in the history of Victorian society and mentality.

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GWYN HARRIES-JENKINS. *The Army in Victorian Society*. (Studies in Social History.) Toronto: University of Toronto Press. 1977. Pp. xi, 320. \$12.50.

Gwyn Harries-Jenkins rightly contends that by 1900 the British army was an anachronism. To explain this development, the author has examined the army officers, their social ideals, and their relationships to the rest of Victorian society. Harries-Jenkins reasons that the officers, drawn predominantly from the conservative landed interests—nobility, landed gentry, and a rural middle class—failed to adapt to the nineteenth-century industrial society. Moreover, in a milieu that emphasized "character" over "intellect," "conformity" over "individuality," the education of officers after they received commissions was often discouraged. As a result, the officers failed to develop a sense of professionalism.

To a certain extent, this book is disappointing. It is too narrowly conceived and is definitely mis-titled; it should be called *The Officer Corps in Victorian Society*. Harries-Jenkins hardly mentions the middle and lower classes, except to point out that neither liked or trusted the army and its officers, whom they perceived as indulging in a whirl of social life while basking in a sea of privileges. The militia, yeomanry, and Volunteers, important to many officers at least socially, are given only passing notice. Nor does the author explain sufficiently how the officers failed to utilize industrial and technological knowledge of the era.

On the whole, the author's research is excellent, with heavy emphasis on government documents and contemporary articles and books. There are, however, some omissions in the use of recent scholarship. The discussions of both professionalism and education would have been broader had he used W. J. Reader's *Professional Men* and Brian Bond's *The Victorian Army and the Staff College, 1854-1914*.

The strongest part of the book deals with the politics of the period. Harries-Jenkins convincingly argues, for example, that Parliament was directly responsible for many shortcomings of the officer corps by failing to delineate clearly the tasks of the army, to provide for adequate pay, or to encourage the development of a general staff and intelligence department. The author provides a clear picture of the terribly complex purchase system, and of the political attitudes and involvement of the officers. He points out that this political involvement, in both local and national government, decreased markedly in the late nineteenth century with the passing of the County Councils Act and through changes in parliamentary practices. Deprived of much direct political participation and decision-

making, officers became increasingly bitter toward the political parties.

LOWELL J. SATRE
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YVONNE KAPP. *Eleanor Marx*. Volumes 1 and 2. New York: Pantheon Books. 1977. Pp. 319; 800. Cloth \$10.00, paper \$4.95; cloth \$17.95, paper \$6.95.

Eleanor Marx (1855-98) was the youngest of the three daughters of Karl Marx, the most talented and the only one to play an important role in the history of the European labor and socialist movements. At a time when interest in both Marxism and feminism has reached a peak, it is natural that attention should be paid to the life of Eleanor Marx, both for its intrinsic interest and importance, and for the light which it sheds on late nineteenth-century social movements.

The first biography of her was a scholarly study by Chushichi Tsuzuki, published in 1967. It was followed by a spirited work of historical fiction by Michael Hastings, published in 1970. The third, and no doubt in our time final, study is by Yvonne Kapp. Her first volume was published in England in 1972, taking the young Eleanor to the death of her father in 1883. The second volume, published at the end of 1976, was followed by an American edition of both volumes in the autumn of 1977.

It is a remarkable story which Yvonne Kapp has to tell. Eleanor Marx was involved with radical and socialist politics from an early age. When she was 13 she was so roused by the Irish struggle that she declared: "Formerly I clung to a man, now I cling to a nation." She was immersed in the British socialist movement of the 1880s, being a leading member of the first British socialist organization, the Social Democratic Federation, which was somewhat insecurely based on Marxist principles. Breaking with its autocratic leader, H. M. Hyndman, in 1884, she joined William Morris and others in the establishment of the SDF's influential but short-lived rival, the Socialist League. She participated in many of the London socialist processions and marches of the 1880s; after the "Bloody Sunday" riot in November 1887 she wrote to a paper that she had been "in the thick of the fight," and Engels wrote of "her coat in tatters, her hat bashed and slashed." She was closely involved with the revived trade-union movement at the end of the 1880s, playing an important role in the gasworkers' union, whose rules and address she was largely responsible for compiling and on whose executive she served. She was also active in the dockers' strike of 1889, and its leaders paid her generous tribute. Uniquely qualified as a participant in the international socialist movement, she

was a founder member of the Second International in 1889 and was particularly active in the movement for an eight-hour day. Only a few weeks before her death she was engaged in another notable struggle, the engineers' lock-out of 1897-98.

In addition to her socialist work, which was inseparable from her committed feminism, Eleanor Marx was also active in literary circles. She lectured and wrote on poetry and drama and did a good deal of translating. Her translation of *Madame Bovary* was for many years the only English version and it is still in print. She also learned Norwegian in order to read Ibsen and translated two of his plays, one of which was in print for over sixty years. She was an enthusiastic advocate of the new literary movements of her day, and her thwarted ambition to become an actress must have been partly satisfied by performances before socialist and private audiences; with Bernard Shaw and Morris' daughter May, for example, she took part in a private presentation of Ibsen's *A Doll's House* in 1885, several years before its first public performance in England.

Eleanor Marx's passionate but unhappy life was complicated and grievously saddened by her long liaison with Edward Aveling, a man of remarkably wide scientific and literary interests with whom she lived in free union (as he was already married) from 1884. Aveling's relationships with other women and his utterly unscrupulous behavior over money have frequently been chronicled. In June 1897, his wife having died several years before, Aveling married a young actress named Eva Frye. Whether it was this which finally overcame Eleanor Marx, as Tsuzuki and others have assumed, or whether it was her own and Aveling's illnesses, the long and tortuous difficulties in which she was embroiled with the German Marxists and, above all, her despair at the insistence of the British labor movement on following the path of gradualism rather than Marxism, as Yvonne Kapp thinks, she finally took her own life on March 31, 1898—a tragic loss to her many friends and comrades and to the European socialist movement.

Yvonne Kapp's is a monumental study, which has rightly received high praise. She has coped with an enormously complex and detailed period and with source materials in several languages. Her two volumes add significantly both to the history of European Marxism and to British social history. Yet for one reader at least, her constant editorializing and open partisanship (with Eleanor Marx and Engels as her twin lodestars) detract seriously from the book which might have been and reduce her scholarship in places to the level of a political tract. Her eleven hundred pages contain much fascinating, important, and poignant material. But the most balanced, judicious, and reliable

account—at a third the length—remains that given a decade ago in Chushichi Tsuzuki's pioneering biography.

DAVID RUBINSTEIN
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E. P. THOMPSON. *William Morris: Romantic to Revolutionary*. Rev. ed. New York: Pantheon Books. 1977. Pp. x, 829. \$17.95.

E. P. Thompson has made it his practice to write about men and women on their own terms, discarding whatever gloss others might have applied to suit their particular ends. His declaration at the beginning of *The Making of the English Working Class*—that he will attempt to rescue his subjects from “the enormous condescension of posterity”—expresses as well the intention of his biography of William Morris, first published in 1955 and now reissued with minor revisions, excisions, and postscript. Thompson despairs of those who have tried to tidy up Morris' politics by sweeping them behind the arras of the arts-and-crafts movement. He is just as impatient with those whose disappointment that Morris did not “become” a Marxist according to their particular orthodoxy has led them to dismiss Morris' own perceptions and theories as unimportant.

Thompson perceives Morris' life as a trajectory, arching from the “subjective romanticism” of Keats through the “sublimated rebellion and despair” of his middle years, then on, by way of the “recuperative societal myths of Icelandic saga” to the socialist revolution. Thompson does not argue that the trajectory was in any way preordained in the direction of its ultimate destination. He takes each segment of the flight seriously, just as Morris himself did, thus avoiding the condescension he condemns in others. Yet if the course was not prescribed, there is something not unfamiliar about its pattern. Morris' trajectory bears at least some resemblance to Friedrich Engels'. Both despaired of the bourgeois world into which they were born. Both reacted, not by retreating into the privacy of romanticism or the ultimate sterility of pre-Raphaelitism, but by crossing what Thompson calls the “river of fire” to revolutionary socialism.

A good deal more of the bourgeois was burnt out of Engels than of Morris in the crossing, however. Thompson treats the passage as a conversion, which he sees as “among the great conversions of the world” (page 243). But Morris, though fully committed to socialism, could not entirely foresake the world he had left behind across the river. There were the copper mines, from which he continued to draw large dividends, and about the conditions of which he does not appear to have

tried to discover very much. There was the firm, dependent upon an increasingly enthusiastic and fashionable bourgeois clientele. Above all, there were the habits of life and of thinking which he could not simply throw off while attempting to acquire a new persona within the revolutionary working class. “One camp or other you have got to join: you must either be a reactionary and be crushed by the progress of the race, and help it that way: or you must join in the march of progress, trample down all opposition, and help it that way.” Easy enough to take that sort of uncompromising stance in a lecture; more difficult to accomplish that drastic transformation, given almost fifty years of conditioning and attachment as a part of the “enemy” camp.

Thompson devotes a good deal of space to a discussion of Morris' “purism,” expressed in that set of doctrines which led the Socialist League to oppose any attempt to cooperate with working-class political movements within the existing system. “Purism” may have come naturally to a middle-class man never quite at ease among working-class comrades. Thompson would probably disagree with that suggestion. He makes the point that Morris was better able to communicate with workers because he had himself enjoyed the experience of his own workshops.

Morris does not appear to have been entirely comfortable as a proletarian, however. He acknowledged and despaired of the distance that, despite his having crossed the river, continued to separate him from the workers. Once grant that distance, and it perhaps becomes easier to understand why “purism” would seem more promising and feel more psychologically comfortable to a former member of the bourgeoisie. The problem was complicated, and Morris never completely resolved it. He vowed that he would not allow “the values and outlook of the middle class to penetrate the Socialist movement.” Yet to keep socialism “pure” in that sense would mean for him the disowning of much from his own past that he had brought with him across the river and reshaped into a socialism that bore his particular mark.

In fact, Thompson's metaphors contradict each other. The trajectory implies a continuum; the river, a break. Thompson's own analysis would seem to sustain the former figure, not the latter. He argues persuasively that the importance of Morris' thought and work lies in the fact of its integration of English romanticism into a revolutionary socialism that was something other than orthodox Marxism. If that is true, we have, in Morris, a case not merely of conversion, but of transformation. Thompson at times appears to want to make his story simpler than that. The impulse is fortunately defeated, however, by his

determination to allow Morris' extraordinary life and writings to tell their own fascinating and complicated tale.

STANDISH MEACHAM
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Austin*

JOHN SPRINGHALL. *Youth, Empire and Society: British Youth Movements, 1883-1940*. Hamden, Conn.: Archon Books. 1977. Pp. 163. \$12.00.

This brief monograph (129 pages of text including footnotes at the end of each chapter) defines a youth movement as one which aims at comprehensiveness in membership, has an explicit ideological framework, and includes a sufficient degree of youth involvement in leadership and organization. The five movements considered—Boys' Brigades, ethnoreligious Lads' Brigades, Cadet Corps, Boy Scouts, and Woodcraft groups—are examined individually. Successive chapters analyze each movement's formation, growth, and decline. Two other chapters investigate the experiences of the Boys' Brigade and the Boy Scouts in Enfield prior to 1914 and the importance of summer camps to the movements.

Some repetition occurs in explaining why leaders wanted to establish the movements, because proponents sometimes shared the same religious, national, and imperial goals. But this almost unavoidable organizational problem and the few typographical errors are minor faults. Several charts on numbers of members, occupational status, and religious denomination as well as biographical notes of the movements' leaders supplement the text. All effectively illustrate the author's view of the movements, with the exception of the woodcraft groups, as "a form of recreation enjoyed by the upper-working-class and the lower-middle-class, who used them as a means of accustoming the membership to accept and to find a place within an evolving urban-industrial society" (p. 126).

But, as Springhall admits, documentation of this point is only one aspect of a study of British youth movements, empire, and society. He acknowledges the need for detailed investigations in three major areas: attitudes and experiences of the youths, female youth movements, and regional and local operations of the movements. Comparative studies, especially between British movements and those in the Dominions, are also in order. Scholars undertaking such studies will be aided by Springhall's useful critical bibliography of printed and manuscript sources.

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ANTHONY P. HAYDON. *Sir Matthew Nathan: British Colonial Governor and Civil Servant*. St. Lucia: University of Queensland Press. 1976. Pp. viii, 280. \$26.65.

It is the contention of Anthony P. Haydon that Sir Matthew Nathan achieved the success he did in the British colonial service because he so admirably reflected the virtues most highly esteemed by his superiors in the Colonial Office. The innovative and imaginative colonial administrator was not prized as highly as the man who could maintain order and a balanced budget. Nathan's career in Sierra Leone, the Gold Coast, Hong Kong, and Natal came in a period of imperial consolidation, when London was more concerned with the survival and retention of the imperial estate than with its expansion. In that atmosphere, Nathan's care and prudence, his sense of obedience to authority, and his keen perception of the mood of those in power in Whitehall made him highly favored there. "We love Governors who run their Colonies without having awkward questions, and so you are very dear to us," one of them wrote to him.

The quality of conformity, of going along, may have been pleasing to Nathan's superiors, but in the long run it contributed to the gravest failure of his career. After years in the colonial service, Nathan was appointed under-secretary of Ireland shortly before the outbreak of war in 1914. In the succeeding months, Nathan "went along" in his usual way with the views of his superiors. He tried to assist in the creation of an atmosphere in which the establishment of Home Rule would seem increasingly plausible, largely by ignoring the signs of resort to violence on the part of the Irish militants. The Easter Rebellion discredited this policy, and those who practiced it, including Nathan and his superior, Augustine Birrell.

Nathan's general unwillingness to disturb the minds of his superiors by the intrusions of any views of his own may perhaps be explained by his being a Jew. This was certainly a handicap to his advance in British military and governmental circles. It could be overcome, as it certainly was, but at a cost of Nathan's individual assertiveness. After all, the family motto was "never request, never refuse."

While this conformity may have brought Nathan both good and ill, it presents his biographer with some problems, the chief of which may be in making his subject interesting. The student of British imperial policy and administration will find aspects of Nathan's career worthy of attention; surely the esteem in which Whitehall held him for a good deal of his career testifies to the measure to which London cherished quiet administration. Yet studies in conformity and, indeed perhaps quasitimidity, lack essential elements for arousing inter-

est. Nor, in this case, does the author's style help. The long convoluted sentences must have occasionally exhausted him as much as they do the reader. But the study is firmly grounded on the Nathan papers, and is obviously the result of workmanlike research.

DONALD C. GORDON
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College Park*

ASA BRIGGS and JOHN SAVILLE, editors. *Essays in Labour History, 1918-1939*. Hamden, Conn.: Archon Books. 1977. Pp. 292. \$15.00.

When George Orwell wrote of the gulf that separated him from the miners and their families of Wigan, he unerringly touched the central theme of working-class history for the interwar years. These essays are about one very important aspect of that gulf. For the working class during the twenties and thirties it was not so much the interclass divisions that mattered as those within the class itself. And throughout this third volume of *Essays in Labour History* there is that depressing and recurring story of the ravine of incomprehension that separated "official" labor, its leaders, institutions, and bureaucracies from its mass base.

The key to understanding the labor movement in the interwar years is to recognize that it was firmly placed within a corporate structure that generated, defined, and sustained its own power and position, so that it no longer needed—nor needed to heed—its rank-and-file constituents. This was a phenomenon that could exist at many different levels and was by no means simply a matter of conspiratorial power groups. Frank Wilkinson, in a complicated but very important essay on collective bargaining in the steel industry, shows how the growing separation of the leadership from the rank and file of the union had to do with the changing economy of the industry and the shifting composition of the work force. But at another level there was the bruiser Ernie Bevin, who smashed not only the sainted Lansbury but, more significantly, those rank and file within his own union who stood for militant trade unionism. And there was the insidious negativism of Jimmy Thomas whose iniquities are confirmed by this book and are now so well known that a revisionist biography cannot be long delayed.

The meanness of spirit of the labor leadership ranged from the petty to the profound: it spent two decades refusing to do anything positive and penalizing those within the movement who tried to suggest constructive answers to the problems of the period. John Saville, in a powerful essay, expands on the obvious failure of "official" labor to

address in any coherent way the central questions of Fascism and unemployment. Margaret Cole and Patrick Seyd examine the failure of various left pressure groups to have any impact on policy-making, blocked as they always were by the suspicions and conservatism of the party and union bureaucracies. John Lovell explores the do-nothingism of the insignificant Special Industrial Committee set up in 1925, laughably, to prepare policy for the TUC in the event of a general strike. But this was by no means the half of it: Alan Deacon shows, as part of a continuing effort to demythologize the history of social policy, that MacDonald's 1931 "betrayal" was no mere aberration. Since 1923 the labor leadership had collaborated in, even initiated, unemployment insurance policies that could be used to penalize and persecute their constituents.

For the most part the essays in this book are interesting for the confirmation they add to what we already know about the twenties and thirties. What the book lacks, and it is a regrettable omission, is a general essay to examine the current state of interwar labor historiography and to point the way forward for research in a period that is evidently becoming a new growth area for labor studies.

RICHARD PRICE
Northern Illinois University

JOHN CAMPBELL. *Lloyd George: The Goat in the Wilderness, 1922-1931*. London: Jonathan Cape. 1977. Pp. xii, 383. £10.00.

When David Lloyd George resigned the British premiership in 1922, King George V confided to his diary his sadness at the loss and his confidence that the *Man Who Won the War* would again hold that high office. There were many in the nation who, happily or not, agreed, and for years thereafter some Englishmen spoke of "when Lloyd George comes back." That time, of course, never came. John Campbell's *The Goat in the Wilderness* is the latest contribution to what Kenneth O. Morgan once called "the Lloyd George industry," and that field of endeavor has lately seen a great deal of attention lavished on the lesser-known periods of the Welshman's life. John Grigg illuminated the early years, before Lloyd George's rise to office, in his *The Young Lloyd George*, while the publication of A. J. Sylvester's diary, admirably edited by Colin Cross, revealed what life with the elder statesman was like, after 1931. Campbell's new book brings into sharp focus the years between 1922 and 1931 (the year in which the MacDonald-Baldwin National Government was created), and he addresses himself primarily to the political circumstances

which made it impossible for Lloyd George to "come back."

The author makes clear the themes he wishes to pursue, and the first, which Grigg's earlier book as well as Michael Kinnear's excellent *Origins of the Lloyd George Coalition* support, was that Lloyd George's reputation for inconsistency and untrustworthiness was vastly overstated and was, in many cases, a purposeful slander nurtured by his enemies. The Welshman always hated and attacked weakness, yet he always sought compromise rather than prolonged struggle. Alas for him, this was easy to twist into a seeming contradiction.

A second interesting position Campbell takes is that much of the blame for the irreparable split in the Liberal Party was due to the refusal of the Asquithians to accept the former Coalitionist Liberals back into the fold in any but the garb of repentant heretics. Even after the retirement and death of Lord Oxford and Asquith, former giants like Lords Grey and Gladstone and Walter Runciman formed the Liberal Council to undermine the authority of Lloyd George, even though he was official leader of the party and held the allegiance of the majority of its members.

Finally, and perhaps most importantly, the author examines closely the evolution of the Keynesian pump-priming economics of the Lloyd Georgian "New Deal" offered up by the Liberal Party in 1929. Beginning with the foundation of the Liberal summer schools, Campbell follows the development of these ideas through the publication of the Liberal *Yellow Book* (which laid out the program in somewhat scholarly language) and of the pamphlet *We Can Conquer Unemployment* (which dealt specifically and in popular terms with the unemployment plans of the program).

The 1929 election was the beginning of the end for Lloyd George; his ties to the party were all but severed two years later, and he began his rustification into elder statesmanhood.

The Goat in the Wilderness is a good book. It is, in effect, a well researched and vividly written tragedy which leaves one with the feeling that it was the British electorate, who voted overwhelmingly for the Baldwins, MacDonalds, and their ilk—not the Goat himself—who were cheated in the 1920s and 30s.

R. J. Q. ADAMS
Texas A&M University

J. A. CROSS. *Sir Samuel Hoare, a Political Biography*. Foreword by R. A. BUTLER. London: Jonathan Cape. 1977. Pp. xiv, 414. £10.00.

Sir Samuel Hoare played a significant part in British politics from 1922 until 1940. He was one of

those Conservative back-benchers who helped to destroy Lloyd George's Coalition. He became, in time, a minister of the crown and held several important posts in government until Winston Churchill, his political enemy, became prime minister in the crisis of May 1940. J. A. Cross' biography contains excellent, if partisan, accounts of the various political developments in which Hoare was involved. His study, based on the Templewood papers and other important archival sources, is, like its subject, prudent, dry, perceptive, and extremely competent.

Hoare and his wife were always obviously ambitious. For a long time his friend Lord Beaverbrook, and others also, could look upon him as a future prime minister. As secretary of state for air, his first ministerial post, he demonstrated high administrative skill and helped to preserve the independence of the Air Ministry and the RAF from the designs of the two older services. His eventual reward was the India Office, a significant advance in his status. In carrying the ministry's Government of India Bill through the House of Commons Hoare won a notable parliamentary triumph. In these debates he challenged Winston Churchill and beat him decisively. Hoare was to pay a bitter price for his victory. When Churchill became prime minister he saw to it that Hoare's great ambition, to serve as viceroy of India, was denied to him. It is no wonder that Lord Butler, in his foreword to the book, refers to Churchill as Hoare's "great enemy."

Hoare's name will always be associated with the notorious Hoare-Laval Pact of December 1935. Cross deals with this episode in an analysis of masterly clarity which makes the best case possible for Hoare's part in the affair. When the details of the arrangement with Laval were made public there was a popular outcry, and Hoare was forced to resign in disgrace. Nevertheless, he was back in office within a few months. From 1937 onward he was one of Neville Chamberlain's principle collaborators in the policy of appeasement. There could, of course, be no place for him in the War Government formed by Churchill in 1940. He was sent to Spain as ambassador, and there he played a significant role in helping to keep Franco from joining in the war. Cross' biography is a genuine contribution to our knowledge of British history in the period.

ALFRED GOLLIN
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Santa Barbara

HENRY FAULK. *Group Captives: The Re-education of German Prisoners of War in Britain, 1945-1948*. London: Chatto and Windus; distributed by Humani-

ties Press, Atlantic Highlands, N.J. 1977. Pp. 233. \$15.00.

Some four hundred thousand German prisoners of war came to Britain during and following World War Two. They presented both a challenge and an opportunity for British officials who hoped, through denazification and a positive program of re-education, to foster tolerance and an understanding of democratic values and procedures in the minds of these prisoners. This book is derived from Henry Faulk's lengthier work, volume eleven, part one (on "re-education") in the general history of German prisoners of war commissioned in 1951 by the German government. Near the war's end he was seconded by the War Office to the Political Intelligence Department, where he organized the re-education of German prisoners of war in Britain. This department, redesignated the Prisoner of War Division of the Foreign Office (POWD) developed the re-education program implemented in British camps from mid-1945 to 1948.

Re-education met with its greatest success among the 200,000 Germans brought to Britain after their capture during the war, success meaning the reorientation of group norms in a democratic direction. Early in 1946 another group of 123,000 prisoners from American camps joined the first. Many believed they were being repatriated; instead they remained in Britain for labor purposes. The most difficult groups came from Canada (33,400) from February 1946 to February 1947 and from camps in Belgium (79,000) during March to August 1946. The Canadian group contained many loyal Afrika Corps members interned early in the war, while the prisoners from Belgium had reacted negatively to conditions in the Belgian camps. Remaining prisoners were transferred from American camps in Britain or were Surrendered Enemy Personnel.

Faulk discusses in a concise, objective and, at times, somewhat dry manner both German and British attitudes affecting re-education, the changing environment and the aims and methods of POWD. He offers a generally favorable evaluation of the results. His sources, many uncovered for the first time, include contemporary documents by the prisoners and POWD staff, other contemporary accounts, and letters from former participants on both sides. The most dramatic accounts deal with prisoner reaction to re-education and, after Christmas 1946, with British public reaction to fraternization.

Faulk has valuable observations to make concerning the success of POWD's social, attitudinal approach to re-education as opposed to the individual, direct political approach; the importance of "inclusive group" (pp. 10-11) norms for psycho-

logical security, especially during the passage from family to group identification described as "psychological puberty" (p. 210); and concludes with the hope that "one day the integration of the young adult will be into an adult world and not simply into an adult inclusive group" (p. 215). This is a thorough book and an enlightening one on the subject of human behavior under stress.

ROBERT W. CARDEN
Curry College

ALEC DOUGLAS-HOME. *The Way the Wind Blows: An Autobiography*. New York: Quadrangle. 1976. Pp. 320. \$10.95.

Like most books of its genre, Alec Douglas-Home's autobiography has little to offer students of British politics or foreign policy. Although he was a cabinet member in every Conservative government between the end of World War II and 1974, holding important positions in the Foreign and Commonwealth Offices as well as serving briefly as prime minister, he reveals next to nothing of the considerations on which the policies he helped to formulate were based. Richard Crossman he is not.

While this is not surprising, it is regrettable, for Home was a participant in or first-hand witness to such major events as the Suez crisis, the Philby affair, and the Profumo scandal, not to mention Britain's remarkably successful disposition of the remains of a once great Empire. What Home offers on these and the other subjects with which he deals is a standard defense of the policies pursued by the governments in which he served. An unabashed cold warrior, he dissertates frequently on the evils of Communism and the duplicity of the Russians. While he is more temperate in his remarks about "the Socialist Party," as those of his political persuasion refer to Labour, he makes it clear that he does not view them as a particularly constructive force in British society.

Home does offer one or two interesting observations when discussing the Rhodesian and South African situations, with which he was closely involved, but these are more than offset by his ruminations on Vietnam, which read like the collected wisdom of the Johnson and Nixon administrations on the subject. Primarily, however, the focus is on his impressions of the world leaders he encountered in the course of his duties. As is so often the case, his observations tell as much about him as they do about the people he is describing.

His politics notwithstanding, Home reveals himself as a thoroughly decent, low key, reasonably perceptive gentleman politician with a quiet sense of humor. Perhaps the most useful bit of wisdom in his book has nothing to do with politics or foreign

policy but derives from his long experience in attending elaborate state dinners. It is quite simply to "eat sparingly at night; . . . and balance each glass of wine with one of water," to avoid suffering the morning after. Reading *The Way the Wind Blows* is like spending the afternoon chatting pleasantly with Alec Douglas-Home about the world as he knew it. Gracious, self-effacing, and mildly amusing, he tells little that you could not otherwise have known.

ROBERT P. SHAY, JR.
New York City

R. DUDLEY EDWARDS. *Ireland in the Age of the Tudors: The Destruction of Hiberno-Norman Civilization*. New York: Barnes and Noble. 1977. Pp. 222. \$17.50.

After a long period of neglect, the general history of Tudor and Stuart Ireland has begun to receive the attention it deserves. R. Dudley Edwards is more than well qualified to make his contribution to this revival. He has chosen to confine his work largely to the Tudor period although there is a brief introduction which looks back as far as Early Christian Ireland and an epilogue that surveys events after the sixteenth century up to 1922. The emphasis, however, is placed on the Reformation, the Roman Catholic reaction, English expansion, and what is called "Catholic Ireland's War." Finally, there is an interesting appendix on the historiography of sixteenth-century Ireland.

The theme of the book is the destruction of the "Hiberno-Norman civilization" under the dual pressure of English expansion and the Counter Reformation, and the method adopted to pursue this theme is a narrative account of politics. At the conclusion of the book the theme is taken too far. We are told that it will be up to a subsequent generation to determine whether "either or neither" of the two present Irish states has perpetuated the ideas of this civilization, but it is not made clear what these ideas were, nor how a twentieth-century state can be expected to perpetuate the ideas of a civilization that is supposed to have been destroyed four hundred years ago.

Fortunately, during the main body of the work the theme is implicit more than explicit, and the stress upon politics permits some important points to be made. Irish sixteenth-century events, for instance, are placed firmly in their European context; thus the link between English attempts to reconquer and colonize Ireland and the fear of French or Spanish intrusion is well illustrated. Ireland became the battleground for two contending forces, neither of which was indigenous. On the internal history of Ireland, there is new material presented on Irish legislation, often

drawn from unprinted sources. There are, however, deficiencies that arise from the concentration on politics, one of them being an imbalance between the general account and the detail used to support it. It is implied that the Irish Parliament acquiesced to the Reformation out of fear, yet it was surely more defiant than its English counterpart when it wanted to be. Brendan Bradshaw's work on the dissolution of the Irish religious orders is cited once in the chapter on the Reformation, but only on a minor point. We are not told about how the monasteries were managed nor why they were defended in Parliament. Similarly, although virtually every act of Parliament passed in Tudor Ireland is cited, little information is supplied on Parliament's changing composition during the period. The specialist, therefore, grateful as he may be for the guidance given on legislation and government personnel will have serious reservations about many of the generalizations. The general reader, on the other hand, will surely be overwhelmed, or at least deterred, by the abundance of minutiae.

M. PERCEVAL-MAXWELL
McGill University

MICHAEL HECHTER. *Internal Colonialism: The Celtic Fringe in British National Development, 1536-1966*. Berkeley and Los Angeles: University of California Press. 1975. Pp. 238. \$15.00.

"Internal colonialism" is a concept first popularized by American black writers to describe the status of American blacks. They perceived American society as divided horizontally into quite rigid strata like colonial society, so that whites always dominated blacks. The theme was then taken up by French Canadian writers, who claimed that there was a similar stratification in Canada, based on language and culture rather than color. Subsequently, the term has been freely used by writers in other countries and is now in danger of becoming little more than a pejorative alternative to centralization or racialism. American blacks were assigned an inferior role in American society because of their color, which, until recently, effectively prevented their assimilation. Whereas European immigrants might in time become assimilated into the dominant American social group, color hindered the assimilation of blacks. In countries peopled chiefly by Europeans, minority groups have not normally been distinguishable by color, and have had a choice between assimilation and cultural self-assertion. Thus, most of the large number of French Canadians in New England have been assimilated into the dominant North American culture; those who stayed in Quebec have

largely resisted such assimilation. So too with the Scots and even the Irish. National movements appear to be primarily a phenomenon of the national birthplace and arouse only relatively faint echoes elsewhere. As to what launches them into politics and gives them electoral victories we still have only the most rudimentary ideas.

Michael Hechter's book will not help historians very much when they come to tackle such questions. His sociological prose is not enlivened by major insights. And there are strange lapses from common sense. For instance, an inconclusive section is devoted to "considering the fate of the Celtic-owned banks" (p. 88)—the Scots did well, the Irish not so well, the Welsh very badly: why the difference?—though there is no evidence that I know of that Scottish or Irish bankers have more than a few drops of Celtic blood in their veins. Hechter does provide a valuable set of statistics which illustrate clearly the economic, social, and religious differences between England, Scotland, Wales, and Ireland, some quite lucid passages on the importance of nationalist movements in the "Celtic fringe," and a very sensible statement of the weaknesses of social science literature with regard to ethnicity. This is a useful beginning, but only a beginning.

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PATRICK BOLGER. *The Irish Co-operative Movement: Its History and Development*. Dublin: Institute of Public Administration. 1977. Pp. xii, 434. Cloth £9.00, paper £4.50.

It is customary in any account of cooperation in the British Isles to refer to the Rochdale Pioneers, that handful of working men who by starting their retail store in 1844 set a pattern for the huge British cooperative movement of later years. The British movement set up its own wholesale agency in 1864 and even acquired farms and tea plantations, but it remained essentially a consumers' organization. In Ireland the pattern was different. There were not the close links between trade unionists and cooperators so evident in Great Britain, and, except in Belfast and some other northern towns, retail cooperation was of minor importance. The history of the Irish movement is primarily the history of agricultural cooperatives, of which the most successful were the creamery societies that manufactured dairy products from the milk supplied by their members.

Patrick Bolger has written the first comprehensive history of cooperation in Ireland from its beginnings to the present day. Previous accounts end in the 1920s or consist of commissioned reports,

personal memoirs, or biographies of leading officials. Bolger's introductory chapters describe early nineteenth-century attempts to organize utopian communities inspired by Robert Owen or William Thompson, but the story proper begins in the 1880s with the unlikely figure of the Honorable Horace Curzon Plunkett, third son of Lord Dun-sany, educated formally at Eton and Oxford and informally for ten years as a cowboy in Powder River Valley, north Wyoming. Plunkett and his fellows set out to deliver impoverished small farmers from the octopus grip of the gombeen man—a general-store owner, loan shark, and, not infrequently, wardheeling politician and ostentatious local pillar of the Church. It was a campaign made difficult by the hostility of petty-bourgeois nationalist party leaders and, from 1907, by the animus of T. W. Russell, Plunkett's successor at the Department of Agriculture. The Anglo-Irish war (1918–21), the partition of Ireland, and the Irish civil war brought fresh troubles, but they were merely a prelude to the lean interwar years. The greatest obstacle was the reluctance of many members to pay share capital in full, coupled with a tendency to divert supplies to merchants who offered better terms in the short run in order to undermine the societies. As Bolger points out, money was not forthcoming to provide the necessary education of members in cooperative principles. But he ends on a more cheerful note, for a veritable renaissance began in the 1960s; cooperative dairy membership now amounts to 80,000 as against the 50 who started in 1889.

This is an excellent work, based on extensive source material and enlivened by the author's experience as agricultural advisor in Ireland and abroad. A bibliography would be a useful addition to a second edition.

JOHN W. BOYLE
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A. N. GALPERN. *The Religions of the People in Sixteenth-Century Champagne*. (Harvard Historical Studies, number 92.) Cambridge, Mass.: Harvard University Press. 1976. Pp. 240. \$19.50.

A book carrying on its dust jacket the imprimatur of LeRoy Ladurie raises hopes and caveats. Exploring the relationship between social change and religious expression in eastern France from the end of the fifteenth to the late sixteenth century, A. N. Galpern touches points of great interest to social and intellectual historians. One question dominates: what needs does religion satisfy, and how does it respond to changes in these needs? The highly associative character of late medieval piety

identifies it (to Galpern) as a reaction to prolonged crisis in society. Preoccupied with their security, people joined fraternities, depended on the poor to enable them to do works of charity, and expected their priests to maintain bonds between the living and the dead, the mortal and the sainted. This was before 1500. After the turn of the century calm and order settled on society until the onset of the French religious wars. During this respite from anxiety the heart went out of religious sculpture, fraternities declined, and mystery plays—singular expressions of late medieval communitarianism—came to an end. All these were supplanted by religious individualism, a style of religious behavior to which social groups not dependent on tightly-knit association responded favorably. These groups were drawn, first, to the reforms emanating from Meaux, then to the Protestants who, taking their support from independent groups and individuals, emphasized autonomous belief as opposed to devotional practice. Catholics, in turn, tried to revive the participatory and processional aspects of their religion. But this attempt failed. Too much had changed in the social and political order. Skepticism prevailed among intellectuals, religious art now reflected the sophisticated tastes of wealthy donors, charity was considered a burden on the rich, mutual help in village and town disintegrated.

This synopsis is too curt to do justice to Galpern's book, but it suggests the schematic nature of his argument. At times it lapses into crude mechanism: "Greater clarity in the political and religious hierarchies [after 1500] made it easier to conceptualize the celestial hierarchy" (p. 94). That religion, like everything else, is attuned to the play of social forces need hardly be argued. What must be demonstrated is the precise nature and location of the connection. Sources for such a demonstration are notoriously difficult to interpret; analysis of the documentary value of sculpture and painting is especially inconclusive. Galpern depends much on the evidence of wills, but he does not face—though he acknowledges—the likelihood that in the face of death a testator was likely to be more conventionally pious than at other times. Such flaws are inherent in the French manner of doing history which Galpern has adopted—like his obligatory geographical overture, whose themes are never taken up again in his study. Putting down this book, one has the feeling that a conceptual and methodological approach less beholden to the *Annales* orthodoxy would have yielded a more rewarding exploration of the ways of popular piety at the opening of the modern era.

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SOLANGE DEYON. *Du loyalisme au refus: Les protestants français et leur Député Général entre la Fronde et la Révocation*. Lille: Université de Lille III. 1976. Pp. 200. 29 fr.

Taking the institution of Deputy General of the French Protestants as her point of departure, Solange Deyon has written a sensitive study of the declining fortunes of "*La Réforme*" in the seventeenth century. The Deputy General was the person through whom the "*gens de la religion prétendue réformée*" communicated with the Crown. At first there were two deputies, chosen by the Reformed at regular intervals and given only short-term mandates. Eventually, in a development that has its parallels in the decline of other representative institutions in France, the Crown chose one deputy who served at royal pleasure. The Protestant confirmation of the choice was so insignificant that six years passed between the designation in 1653 of the last deputy, Henri de Ruvigny, and his formal acceptance by the synod of Loudun.

While exploring the functions of the Deputy General and the provincial synods as institutional links between the Reformed minority and the Crown, Deyon focuses upon two questions. She asks, first, whether Ruvigny, who in addition to being Deputy for twenty-five years also served Louis XIV as a diplomat in England, had a more negative than positive influence upon the fate of his coreligionists. Second, she wonders to what extent the relative docility of the Reformed as they suffered persecution helped to bring about their own downfall. Both questions are explored with an appreciation for the nuances of human motivation and the tragic ironies inherent in complex situations. When the Reformed behaved "loyally" their enemies were quick to step up pressures to absorb them into the mainstream of French Catholic culture; when they protested more volubly and even violently the state found new excuses to suppress these "rebels" against God and country. In these circumstances, Deyon asserts, the Reformed behaved as heroically as they could: who would choose between the Last of the Just and the insurgents of Warsaw? As for Ruvigny, he proved his ultimate loyalties by choosing exile to England over a favored place at home after the revocation of the Edict of Nantes. Deyon feels that he never compromised his belief in the possibility of remaining loyal both to his religion and his king, and that he acted with probity and discretion in extremely delicate matters.

If there is one disturbing feature of this book it is that the author found it difficult to integrate the facts of Ruvigny's life with the broader picture of French Protestantism. It is as if she could not quite decide whether to write biography or institutional

history. In her stirring conclusion, however, matters come together as she shows the interconnections of institutional change, diplomatic questions, and the desires of individuals. In all, this is a provocative book.

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MICHEL VOVELLE. *Les métamorphoses de la fête en Provence de 1750 à 1820*. With the collaboration of MIREILLE MEYER and DANIELLE RUA. (Bibliothèque d'Ethnologie Historique.) Paris: Aubier/Flammarion. 1976. Pp. 300.

This study is the third major work by the prolific Michel Vovelle to appear within the last few years. Like its predecessors, *Piété baroque et déchristianization* and *Religion et révolution*, it attempts to trace, by *annaliste* methods of local study and quantitative analysis, the profound transformation in the popular mentality and outlook of the French which Vovelle is convinced took place in the last decades of the eighteenth century. Again Vovelle concentrates his research in Provence, but here he takes as his subject popular festivals, analyzing their transformation from the saints' day processions and *charivaris* of the Old Regime to the officially choreographed Liberty tree plantings and celebrations of Reason held during the Revolution.

Drawing on accounts of travelers and local antiquaries for the Old Regime, and government ordinances and statistics for the Revolutionary era, Vovelle analyzes the types, incidence, location, actors, and symbolism of *fêtes* in the two periods. He finds that when the revolutionaries suppressed folk festivals in favor of official celebrations of Reason, revolutionary martyrs, old age, and the National Guard, they destroyed an institution already in decline. He argues that in the last decades of the Old Regime *fêtes* were losing their significance in popular culture, becoming increasingly secularized, institutionalized, commonplace, and banal. This fits neatly (perhaps too neatly?) with the gradual dechristianization and demystification of the popular mentality after 1750 which Vovelle documented in his earlier books. And he suggests that although the Restoration resurrected traditional *fêtes*, these were artificial growths, deliberately picturesque and folkloric, but no longer true expressions of popular feeling.

Vovelle must be commended for bringing solid statistics and the perspective of the *longue durée* to a topic whose treatment is often confined to recon-dite ethnographic analysis. But perhaps he errs too far in the other direction. One cannot argue, as Vovelle does, that changes in the *fête* reflect

changes in popular mentality without saying something about the role of the *fête* in popular culture, as an instrument for community integration and the enforcement of social norms. Yet these aspects are totally neglected. And elements of his interpretation are questionable. Nineteenth-century specialists, for example, might challenge his conclusion that the popular *fête* died with the Revolution, arguing with Eugen Weber that *fêtes* continued to be a vital part of peasant community life until 1900. And revolutionary historians might wonder about an issue which Vovelle raises but never really answers: were revolutionary *fêtes* directed or spontaneous? Probably there was little that was "popular" about these vehicles of official propaganda; their Old Regime ancestors were less Vovelle's peasant *bravades* and *roumerages* than the official pageants and ballets of Versailles. It is as political instruments that they should be analyzed; one would like to know more about their effectiveness in this role. Here Mona Ozouf's recent study is far superior to Vovelle's. In sum, this book is not the masterpiece *Piété baroque* was. But even a flawed work by a historian as good as Vovelle is worth reading, and this is a major study of an important aspect of popular culture.

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MAURICE AGULHON. *Le cercle dans la France bourgeoise, 1810-1848: Étude d'une mutation de sociabilité*. (Cahiers des Annales, number 36.) Paris: Armand Colin. 1977. Pp. 105.

Until now, Maurice Agulhon notes, the study of French gentlemen's clubs (*cercles*) has been fragmented: political clubs have been studied by political historians, learned societies by intellectual historians, and so on. This brief volume in the *Cahiers des Annales* traces the rise of clubs in France from 1810 to 1848 and shows their contribution to collective life. Agulhon sees this book as part of a projected general history of sociability that will use the methods of psychology and sociology to analyze the origins and development of the democratic spirit.

Clubs of all sorts sprang up rapidly in France after the 1830 Revolution, ranging from literary societies and professional associations to formalized gatherings of the regular customers of a café. Agulhon includes all types of clubs in his analysis; he leaves out only groups like the Jacobin Club which put politics ahead of sociability. It is difficult to generalize about such a diverse subject; Agulhon might better have separated small-town clubs (where men from many professions and interest groups mixed together) from big-city clubs

(which generally reinforced the exclusiveness of each profession and interest group). But Agulhon's generalizations about the club as a form have all the brilliance, perception, and care that we have come to expect from him.

The gentlemen's club, according to Agulhon, was a step forward in the democratization of French life. Before 1830 French society had been dominated by the *salons*, with their implied patron-client relationship between host and guests. But even the most exclusive clubs were democratic republics that elected their officers from among equal members.

Whereas women had played an important role in the *salons*, they were excluded from most clubs, but Agulhon notes that the rise of the clubs caused only a limited loss of place for women. Most club members were rising new men who had never been invited to *salons*. They came from hard-pressed families that could educate only male children. Married to uneducated, uninformed women, they fled from their homes to the cafés where they organized clubs. Thus the clubs were not driving a new wedge between the sexes; they were just reinforcing an old one.

Most important, Agulhon relates the growth of clubs to the rise of liberalism in France after 1830. He contends that the clubs were "implicitly liberal" (p. 70), providing channels through which liberal ideas could pass. But the Paris clubs he mentions—the *Union*, the *Jockey-Club*, the *Cercle agricole*—were all reactionary, even legitimist (pp. 38, 70), while his "typical" provincial club subscribed in 1843 to a broad spectrum of newspapers ranging from the legitimist *Gazette de France* to the republican *National*. Thus the liberalism of the clubs remains unproven. Whether implicitly liberal or not, the clubs were what their members made them.

The clubs were only a form; it was their membership and goals that mattered. Political clubs were part of political movements, each professional society was part of the history of its profession, and so on. But thanks to Maurice Agulhon, historians will be more aware of the club as a form and of its contributions to the collectivization and democratization of French society.

EDGAR LEON NEWMAN
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YVES-MARIE HILAIRE. *Une chrétienté au XIX^e siècle? La vie religieuse des populations du diocèse d'Arras (1840-1914)*. In two volumes. Preface by LOUIS GIRARD. Lille: Publications de l'Université de Lille III. 1977. Pp. 1,017.

The history of the Catholic Church in France during the last three-quarters of the nineteenth cen-

tury can no longer be viewed solely as a series of rearguard actions against an increasingly implacable secular world. Jean Marie Mayeur and Charles Molette, among others, have shown the clergy and laity to be innovative and thoughtful as well as intransigent and merely devout in confronting the problems besetting the Church. But there is still a tendency to regard the situation of religion in France from the vantage point of either the *Ministère des Cultes* or the *Archevêché* in Paris where anticlericalism was loudest and mass dechristianization most obvious.

In *Une chrétienté au XIX^e siècle?* Yves-Marie Hilaire presents a different perspective, the analysis of religious life in the diocese of Arras from 1840 to 1914. In analyzing why the Pas-de-Calais, a relatively "modern" area of France, remained a bastion of Catholicism, he joins a thorough history of the diocese and its clergy to a rigorous and imaginative dissection of the parishioners. It is religious sociology—dug out of national, departmental, diocesan, and parochial archives and replete with the entire apparatus of the *Annales* school—combined with psychology to provide a fascinating account of the relationships between official worship and the popular culture of festivals and pilgrimages.

The ecclesiastical hierarchy considered Arras so important a post that four times between 1851 and 1914 it elected to translate an already proven bishop to the diocese rather than risk the elevation of an unknown. Their caution was rewarded. At a time when most bishops resigned themselves to pessimism, those of the Pas-de-Calais were paladins of the faith. Far ahead of most of the French episcopacy, Pierre Louis Parisi, bishop of Arras from 1851 to 1866, carefully and tactfully implemented the Roman Rite and ultramontanist over the protests of the Gallican elements of his clergy and traditionalists among the laity. With great success he revived popular Christian fervor by sending out preaching missions to miners, industrial workers, and peasants; exploiting the vogue among women of Mariolatry; and taking charge of "clericalizing" the remarkable enthusiasm among all classes for pilgrimages and the construction of calvaries and chapels, 1,035 of which were erected between 1851 and 1895. Considerably less wisely, he failed to comprehend that by taking advantage of the Second Empire's acquiescence in clericalism, which did allow the Church extraordinary powers, he left it open to savage attack when the opponents of authoritarian government finally triumphed in 1877. Coming to terms with the antagonistic Third Republic was the task of Alfred Williez, who held the episcopal chair from 1893 to 1911. He persuaded his monarchist clergy to accept the *Ralliement*, ironically aided by the ultramontanist

dating from the Bonapartist Parisis, and encouraged them to adopt the ideals of *Rerum novarum* in trying to bring back to the Church miners and urban workers whom his predecessors had allowed to drift away.

The administrations of Parisis and Williez, while the most outstanding, were also typical of the experience of the Pas-de-Calais during the nineteenth century. Throughout the seventy-five years Hilaire studies, the diocese was known for its solidity of clerical organization, the early spread and deep entrenchment of ultramontanism, and the ability of the clergy to adapt the message of Catholicism to local circumstances. Here lies most of the reason why the region remained so markedly Catholic—but only in comparison to the rest of France. A vigorous new clergy, younger, poorer, and more rural, was recruited, but it repelled some by its rigor. The bourgeoisie was polarized in religious attraction, many drawn by Romanticism and their fear of social revolution, others rejecting the Church's authoritarianism. The Roman Question of the 1860s galvanized the fervent but led to fears of excessive Catholic influence within the government. All of these issues had national aspects, but Hilaire rarely raises his gaze from the diocese of Arras. It is not a serious weakness in a book of many strengths.

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R. D. ANDERSON. *France, 1870-1914: Politics and Society*. Boston: Routledge and Kegan Paul. 1977. Pp. viii, 215. \$12.50.

Robert D. Anderson of the University of Edinburgh has written a concise and useful analysis of the Third Republic before World War I. The book is primarily a synthesis of the monographs which have proliferated in the past two decades. Anderson has correctly recognized that numerous specialized studies in economic, social, and religious history and electoral geography have made possible a more nuanced interpretation of the significant period when French democracy finally took root. At the same time Anderson rightly observes that the Republic's political institutions have only just begun to attract renewed attention. By interpreting the politics of the Republic within the context of the original research done in other areas, he believes he has written a book which is "more original than might at first appear" (p. vii).

Does Anderson give us a fresh interpretation of the early Third Republic? He is certainly to be commended for emphasizing the complexity of the forces which shaped the Republic. He provides a brief but competent account of the measured in-

dustrial development of France which resulted in more numerous but less disruptive social divisions than elsewhere in Europe. He discusses the regional variations in land ownership, historical experiences, and religious practices which electoral geographers have associated with variations in political behavior. He deals also with the effect of political leaders and their organizations upon political institutions. Against this background Anderson depicts the emergence of a parliamentary system which provided France with an effective forum for its varied interests and less satisfactory legislative machinery. He depicts too the adequate if not perfect integration of political groups, including the Right and the Socialists, into the parliamentary system. Finally he describes the Republic's foreign and colonial policy as skillful and prudent.

The result is a more positive appraisal of the Third Republic than those to which we have become accustomed. Anderson rejects the view of contemporary sociological critics that the Republic was the inadequate government of a stagnant and "stalemate society." Rather, his examination of the most recent scholarship brings him closer to the older laudatory view of the political historian Charles Seignobos. This does not make Anderson's work invalid or superfluous. If anything he does not go far enough in his positive appraisal. Anderson acknowledges a major latent "weakness" of the early Republic which the difficulties of the interwar years exposed: "ministerial instability" caused by "the absence of a strong party system." Anderson fails to recognize that the "weakness" resulted from the need to develop effective alternative governing choices. This need confronts all democracies and requires time and constant experimentation. The early Republic, therefore, should not be viewed as the incubation period of a chronically ill regime. It should be re-examined as the formative period of an evolving democracy.

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PATRICK FRIDENSON, editor. *1914-1918: L'autre front*. (Cahiers du "Mouvement Social," number 2.) Paris: Éditions Ouvrières. 1977. Pp. 235. 54 fr.

Although no overt theme ties together the nine articles that comprise this anthology on the French home front, one name keeps appearing: Albert Thomas, first undersecretary of state for artillery and munitions, then minister of armaments. The policies of the French government in regard to industrial mobilization were, these authors argue, by and large policies formulated by Thomas.

Articles by Gerd Hardach, "*La mobilisation industrielle en 1914-1918: Production, planification et idéologie*," and by Alain Hennebicque, "*Albert Thomas et la régime des usines de guerre, 1915-1917*," are the axis of the volume. Whereas Hardach begins from a view of the total economy evolving toward "state capitalism" and Hennebicque's viewpoint is that of a biographer, both see Thomas as the chief proponent of a planned economy, the concentration of industrial capacity in large cartels and trusts, the reduction of employer-employee conflict through both the militarization of the labor force in key industries and the introduction of arbitration committees representative of both labor and management, and, finally, close cooperation between the state and private industry for their mutual benefit. Thomas and other French leftists had espoused this semicorporatist vision before 1914. Even Alphonse Merrheim had urged the state to favor large industrial firms so as to create conditions necessary for a stronger and more "advanced" labor force.

The Hardach and Hennebicque articles are followed by four other studies of the wartime economy. Two deal with the war's effect on specific industries, the other two with aspects of working-class life. In a study of the carbide industry, Robert O. Paxton illustrates how Thomas' policies favoring industrial concentration were applied to this industry in spite of demands by the "individualists of the Center-Left" in the Chamber of Deputies that the government prosecute the carbide industry under antitrust laws. James M. Laux's companion article describes the expansion of one firm into a conglomerate during and immediately after the war. This motor manufacturer was the beneficiary of Thomas' policy of placing government orders for material only with the largest and most efficient firms.

Mathilde Dubesset, Françoise Thebaud, and Catherine Vincent explore the impact of the wartime experience on women in a study of "*Les munitionnettes de la Seine*." Women did not acquire new skills because factories were reorganized to place them in the jobs requiring the least training. Moreover, most of these jobs were phased out at the end of the war. Once again Thomas set the theme, advocating "equal pay for equal work" but with the proviso that an employer could deduct the cost of training, supervision, and other special expenses from the women's salaries. The result was that women did not share fully in the salary increases enjoyed by most male workers. Thomas' theories dominate Gilbert Hatry's article, "*Les délégués d'atelier aux usines Renault*." Thomas was determined that class conflict be avoided. One means to this end was an administrative order requiring that arbitration committees composed of represen-

tatives of both labor and management be constituted in large factories. Thomas was not willing, however, to compel employers to bow to the recommendations of these committees; and the employers, at least in the Renault plants, either fired the workers' delegates to these committees or had them transferred to the battle front when they protested management policies.

The remaining articles use interesting methods of analysis but are of narrow focus. Jean-Jacques Becker's study of reactions to the outbreak of the war in the department of Charente convincingly argues that the war was not welcomed there but caused a fearful and cautious anxiety. In another local study, Louis Koll shows how the German occupation affected one small town in Lorraine. Finally, Serge Bernstein argues that the assimilation of the Radical Party into the *Union sacrée* brought the end of the party's role as an ideological, although not as a political, force.

Containing excellent examples of scholarship and analysis, this anthology will be of benefit to all historians of twentieth-century France.

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SIMONE PÉTREMENT. *Simone Weil: A Life*. Translated by RAYMOND ROSENTHAL. New York: Pantheon Books. 1976. Pp. xiv, 577. \$15.00.

This very long biography of a very short life is rewarding to the persevering reader who can overcome irritation at the repetitiveness and the occasionally ungraceful style (not entirely a fault of the translator). It involves the reader in a life that may be called exemplary for the attempt—passionate but finally doomed to fail—of a woman to accord her life with her ideas. She failed because she did not give life its due, but not before having applied her prodigious intelligence to many social and political questions of her time and to the spiritual salvation of her soul.

Many readers will regret Simone Pétrement's choice not to write a "life and times" and her failure to analyze Simone Weil's work in depth (too often Pétrement's comments are reduced to paraphrases of quoted passages), but this chronology of a life is invaluable and should be read by all those for whom Simone Weil has become an object of fascination.

Weil was a radical and an extremist because nothing ever stopped her from going to the root of a problem. Whether she affirmed that defeat without war was preferable to a victorious war, or recommended the abolition of all political parties in order to achieve greater democracy, or insisted that the Church should modify the conditions un-

der which it grants sacraments so that she could belong to it without violating her intelligence, her views in context are always deeply moving because, in spite of much paradox, there is never any doubt as to her sincerity. She was one of those rare free spirits who examine all the more critically that which appeals naturally to their sympathies. Thus she clearly saw the contradiction in Marx's theory of revolution, spoke out against many aspects of Communism, trade unionism, and Christianity, and predicted the fascistic fate of Gaullism.

Simone Weil's writings are often obscure because she tries to follow without compromise the complexities of the truth rather than superimpose arbitrary order on it. But when her insights are clear her style becomes as lapidary as La Rochefoucauld's or Pascal's: "The necessity for a power is tangible, palpable, because life cannot be lived without order; but the allocation of power is arbitrary because all men are alike, or very nearly. Yet power must not seem to be arbitrarily allocated, yet without the arbitrary there is no power. Therefore prestige, which is basically illusion, is of the essence of power" (p. 298).

Had Simone Weil been a pure scholar, she would have made significant contributions to knowledge (cf. her study on the *Iliad*). Her attempt at a life of praxis is well worth knowing in all its arduousness, and there can be no doubt that Pétrement finally succeeds in making the reader participate in her sympathy for this heretic saint. It would be unfortunate if Simone Weil were to become just another abstraction in the legitimation process of our disjointed times or an object of ancestral worship. But getting to know her as she really was—a mixture of naiveté and sophistication, of intolerance and Christ-like forbearance—with revealing lacunae in her unusually wide-ranging interests (not a word on Freud or Picasso, no real understanding of existentialism)—is a deeply moving and exhilarating experience.

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NICOLÁS SÁNCHEZ-ALBORNOZ. *Jalones en la modernización de España*. Barcelona: Editorial Ariel. 1975. Pp. 180.

This book consists of four articles of varying length and methodology dealing with Spanish development and modernization (the two are not necessarily synonymous). The first two deal with the failure of Spain to achieve sustained economic growth in the middle decades of the nineteenth century, the last two with the timing of fundamen-

tal watersheds in Spain's economic and social evolution. The book itself is interesting in that it is part of a long series of widely sold, cheap, pocket-sized paperbacks, the success of which implies continuing interest by a sizeable educated Spanish public in scholarly analysis of the historical underpinnings of current national problems.

The first essay discusses the formation of the Spanish banking system, 1856-68, with a brief note on Portugal. Primarily a synthesis, it reviews recent work on the topic, outlines the development of banking per se and of credit- and investment-related institutions in Spain, and discusses some of the implications of their development. Banking, closely related to railroads, developed at a rapid rate for about twenty years, involving a substantial amount of foreign investment. Banks, investment or otherwise, had to compete for capital with state-run savings institutions and massive sale of public lands and government bonds. The result—conditioned by the well-developed habit of savers and investors to look to land and the state for security, by the persistence of large government deficits, and by the commercial (as opposed to industrial) orientation of entrepreneurs—was an unproductive mobilization of capital. Investment flowed into land, into an over-built and under-utilized rail system, and into state deposit banks which then invested in government bonds. Very little capital went to increase productivity in industry or agriculture, hence the stagnation of the mid-1860s and the inability of the economy to achieve sustained growth. On the brighter side, Spain avoided having its banking system fall under foreign control, and by the fourth quarter of the century it was basically a national operation.

The second article details the rapid expansion of the postal system after 1854 and its stagnation, again in the mid-1860s. The story, with that of banking, reveals the weaknesses of Jovellanos' eighteenth-century liberalism as applied—a program of removing obstacles to market-oriented resource allocation which assumed that productivity would grow automatically.

The last two articles deal with aspects of Spain's partial transition to twentieth-century modernity. The first documents the development of a national market in wheat. It uses a half century of prices from forty-nine provinces, develops coefficients of variation from the national mean for each province and year, and correlates the regional variations over time. The results show that the autonomous regional wheat markets of the old regime gave way to a national market under most supply conditions only in the last quarter of the nineteenth century. In the final article, the author uses a similar approach in analyzing regional and seasonal variations in births and deaths. He effectively dates

Spain's transition from a society living "by instinct," with births and deaths reflecting age-old seasonal patterns, to a society of "awareness" in which births are fairly evenly distributed through the year and in which public health and food distribution have reversed the seasonal pattern of death. The link to the preceding piece is obvious. By the criteria of markets and demographic behavior, Spain achieved important aspects of modernity by the end of the nineteenth century, raising important questions about its level of development relative to Italy and France.

The first and last pieces do not appear to have been published before. The second appeared in a different and less effective form in *Moneda y Crédito* in 1970, and the third piece summarizes some of the implications of the author's multivolume project on agrarian prices in Spain in the second half of the nineteenth century, currently being published by the Bank of Spain.

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HOWARD J. WIARDA. *Corporatism and Development: The Portuguese Experience*. Amherst: University of Massachusetts Press. 1977. Pp. xiii, 447. \$20.00.

Howard J. Wiarda, a political scientist, has written an important book on a neglected topic. It is a contribution to the study of twentieth-century Portugal which will invite discussion from Iberianists as well as Latin Americanists interested in comparative history and politics. Even with its flaws, which are not few, Wiarda's heavy tome is the most complete and comprehensive scholarly study in English of Portuguese corporatism during the period 1933-74. His work complements Manuel Lucena's study of the Lusitanian corporatist system, in which governing bodies are selected by indirectly elected functional representatives from occupational groups.

In a thorough way, the author traces the origins of corporatism at large, and in Portugal in particular, from ancient and medieval times to the early 1930s, when the Salazar regime adopted corporatism as a part of its governing system. After three background chapters, chapters four through six analyze Portuguese corporatism during the most vigorous years of the "New State" dictatorship, 1933-45. Chapters seven and eight discuss the 1950s and 1960s up to the physical collapse of Salazar in 1968, as well as a brief era of corporate revival in the mid-1950s. The concluding sections deal with the Caetano regime and corporatism from 1968 to 1974, examining the inner history of

how politics worked beneath the facades. Finally, the author presents a rather inflated chapter of conclusions.

Political scientists and sociologists may indulge in jargon wars over Wiarda's Latin Americanist approach to Portuguese corporatism. But historians should be grateful for two distinct contributions of the book: lengthy treatments of two different eras of the Portuguese dictatorship: the 1930s, when corporatism was established but not in working order; and the 1968-74 era, when, under Salazar's successor Caetano, there was an attempt to make corporatism work in a way which might "liberalize" authoritarian rule.

The materials used in this study are extensive: interviews with corporatist officials under the Caetano regime (1972-74), official publications of the government, and a wide range of secondary literature. An important addition might be the unpublished archives of the relevant ministries, if they become available to the public in future years. Also worthy of comment are the extensive notes, full bibliography, hints on interviewing techniques in Portugal, and appendixes on corporatist structure and membership.

Wiarda is adept at presenting a mass of generalizations balanced with disclaimers. Even tolerating undefined terms and a prolix style, I must note sentences which lack precision, particularly one instance in the conclusion. Having suggested that after Portugal's 1974 revolution the country would never be the same again, Wiarda writes that Portugal would remain, "strongly elitist, hierarchical, patrimonialist, authoritarian, statist, bureaucratic, and, yes, corporatist, to its core" (p. 324).

Although he is not wedded to any one political "model," he retains a theoretical and somewhat didactic view of the problem of change and continuity in Portugal. The study's major theses are threaded with contradictions. After stating that corporatism in Portugal never really functioned until 1956, that it was never the heart of the system, that it never worked as intended, that its failures were greater than its successes, the author then asserts that corporatism "locked" Portugal in the 1930s into a backward and undeveloped system and forced Portugal to remain behind the rest of Europe (p. 334). The author also tends to make generalizations about conditions under the late Caetano era (1972-74), which he knows best from personal experience, and apply them to Portugal before 1968 and after 1974.

Wiarda's study indeed raises more questions than it answers. Yet it is worthy of serious attention, for it is a pioneering contribution to knowledge about Portugal since 1930.

DOUGLAS L. WHEELER
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SIMON SCHAMA. *Patriots and Liberators: Revolution in the Netherlands, 1780–1813*. New York: Alfred A. Knopf. 1977. Pp. xix, 745. \$20.00.

This is a fine book—essentially a political history of the Dutch Republic/Kingdom in the French Revolutionary–Napoleonic period. Simon Schama's rationale for doing "old fashioned" history may be logically askew: "We are too crowded a profession to entrench ourselves in pedantic specializations. . . ." But the result is outstanding. It is written largely from Dutch archival and printed primary sources, with due reference to the French, and on the premise that good studies of Europe *outré France* in this era are sadly lacking. I could not agree more. Much has been done to remedy this deficiency since 1945 by Italians and Germans, and more recently, East Europeans. The Dutch, however, have remained captives of the attitude of the late Pieter Geyl, who tended to assume that the years of French domination (1795–1813) were as dark as those of Nazi occupation during World War II.

Schama argues convincingly that the period 1795–1813 was a watershed in Dutch history—specifically that the foundations of a modern, bureaucratic, centralized state were laid on the ruins of the old oligarchic Republic of the United Provinces—and *by Dutchmen*. He rather backs into this idea in his preface: "This account is written with the presumption that to discount the 'eighteenth-century Dutch revolution' as the shuttlecock of the Franco-British conflict is to tell less than half the story." But he is more blunt about it later. His focus, except during the period 1787–95, when many of his Patriots were in exile, is on internal affairs in the Netherlands. His attention to the details of political maneuvering sometimes approaches overkill, but the story is fascinating.

The work is organized chronologically. Part one deals with the Old Regime in the Dutch Republic and the Patriot Revolt of 1781–87. Part two (half of the book) is devoted to the Dutch and the French Revolution, the establishment of the Batavian "sister" Republic of France, and its history to 1805. Part three is concerned with Napoleon's governments and ends with an epilogue on the restoration of the Prince of Orange.

The Patriot Revolt of 1781–87 was finally crushed by Prussian troops called in by the stadholder. Derk van der Capellen, whose pamphlet had fired off the rebellion, did not live to see the Batavian Republic established in 1795 by French bayonets. Those who did were disillusioned. The "centrists"—Schama's true patriots—lost their great leader, Pieter Paulus, in 1796, and federalists assumed wobbly dominance. Johan Valckenauer left politics and became a diplomat. Pieter

Vreede's "Jacobins" breathed life into the Directory during the first half of 1798 but were purged by another "Man of '87," General Herman Daendels. The author insists persuasively that Daendels was not a "Dutch Bonaparte," but his coup opened the door for a reactionary directory of "patriot regents" (members of the old oligarchy). There followed Napoleon's governments—the disguised dictatorship of Grand Pensionary Rutger Jan Schimmelpenninck (1805–06), the Kingdom of Holland under Louis Bonaparte (1806–10), and direct French control (1810–13). Throughout, the Dutch were robbed of their wealth and had their commerce and industry reduced to shambles.

According to Schama, however, it was not a period of national disaster. As early as 1781, Van der Capellen had said foreign aid would be necessary to enable the progressive minority to break the hold of tradition on the complacent Dutch—their faith in local autonomy, the guilds, the Reformed Church, etc. Though not quite as he had envisioned, foreign force arrived with the French armies. Schama attributes the most cynical of motives to the governments which sent them, noting their rapaciousness. Nevertheless, their presence allowed forward-looking Dutchmen gradually to install the machinery of modern bureaucratic government. "The experiences of . . . [1795–1813] had wrought irreversible changes in the public life of the Dutch which King William [after his restoration] . . . acknowledged and legitimised."

Schama's particular hero is Isaac Gogel, who labored unceasingly under the Republic, the Grand Pensionary, and King Louis Bonaparte to institute a uniform system of national taxes and centralize their administration. He also gives high marks to Schimmelpenninck, an aristocrat who established a "government of all parties," including mossbacks of the Old Regime, but who nonetheless advanced the cause of unitary government.

He is very kind to Louis Bonaparte, as most Dutch historians have been, but not for the usual reasons. The king's government, he notes, had more republican patriots in high places than any before it. He believes that Louis' reign was a turning point in Dutch history in that Louis pressed to completion projects begun earlier which might have collapsed—Gogel's tax reforms, a national administration of dikes and canals (*Waterstaat*), abolition of the guilds, codification of the Dutch law, and centralization of control of an elementary school system which became a model for Europe. Schama gives Louis credit for more energy than he had, I believe, but there is no doubt that he backed reformers and deeply involved the national power in provincial and municipal affairs. Louis' respect for the Dutch legislature, which was maddening to Napoleon, is played down. Perhaps to one with a

fixation on bureaucracy-building this seems unimportant. It was a factor in Louis' downfall, however, along with his resistance to installing the *Code Napoléon* and instituting conscription. His "patriotic" resistance to the Continental System, which the author emphasizes, was, of course, the major sin which led to his deposition.

I am not quite ready to accept the verdict of J. H. Plumb, Schama's mentor, that this book establishes him as "the outstanding historian of his generation." At thirty-two, he seems well ahead of the pack, but I would hope for more and better from him. For one thing, he might take Bertrand Russell's tip that something about style can be learned from Baedeker's guidebooks. In this book, many gems of interpretation, and sometimes essential facts, are buried in tangles of verbiage. He is also a little weak on the general European context, and his characterizations, though striking, are not always accurate.

All in all, however, this is a superb book—deeply researched, well organized, thorough, analytical, and original. It is one-of-its-kind in English and rivals anything I know of in Dutch or any other language.

OWEN CONNELLY
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JØRGEN H. BARFOD. *Niels Juel: Liv og gerning i den danske søetat* [Niels Juel: Life and Career in the Danish Maritime Service]. (Skrifter udgivet af Jysk Selskab for Historie, number 32.) Århus: Universitetsforlaget i Aarhus. 1977. Pp. 411. 92 KR.

Niels Juel is one of Denmark's few naval heroes. He commanded the Danish fleet when it won an important sea battle against the Swedes on July 1, 1677. The Skåne War (1676–79) was one in a long series of wars between Denmark and Sweden over control of the Baltic. The break-up of the Kalmar Union in 1523 had marked the end of Danish hegemony in that region. The Kalmar War, one hundred years later, was the last time Denmark even came close to reasserting its power. Then for a brief moment in the 1670s, when Juel occupied Gotland and defeated the Swedish navy, it must have seemed to the Danes that after one hundred and fifty years, the defeat of the Swedes was close at hand. But what had been won in battle was lost at the negotiating table, since Sweden's ally, France, forced Denmark to surrender all of its gains.

Still, Niels Juel became a popular hero after defeating the archenemy. Yet, despite his hero status, Juel has not been the subject of a biography until Jørgen H. Barfod's. The task has been complicated, according to Barfod, because Juel left no

private correspondence. There is copious material available in the Admiralty's archives, and Barfod makes extensive use of this. Unfortunately, this gives the biography a very "official" character. One never really gets to know the man Niels Juel.

Barfod rarely wanders far from his subject's immediate presence. He says he is simply portraying the situation as Juel experienced it, but as Lieutenant General Admiral and member of the Admiralty, Juel must have had access to information about the overall war situation, information that extended beyond the waters surrounding the fleet. None of this is apparent in Barfod's account. No attempt is made to place Juel, the navy, or even the fighting, within any larger context. This is in the tradition of Danish scholarship, which never ventures far from its sources and hesitates to make assumptions or draw conclusions. Barfod simply sets out "the facts," leaving the reader to sort through and sift out what is important. Every scrap of information is included; every detail is given absolutely equal weight.

It is ironic that Juel, who constantly battled with the Copenhagen bureaucracy in trying to create a first-rate navy, should finally have his story told with the materials and in the tone of that same bureaucracy. Sadly, the biographer lacks the vision and imagination which would make his subject worth writing about.

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FRANKLIN D. SCOTT. *Sweden: The Nation's History*. Minneapolis: University of Minnesota Press. 1977. Pp. xviii, 654. \$25.00.

To the reviewer's knowledge, this is the first history of Sweden by an American scholar since 1931. It represents his life's work and is the most complete account in English of Sweden's history. Franklin D. Scott is emeritus professor of history from Northwestern University and is currently curator of the Nordic Collections at the Honnold Library of the Claremont Colleges. For a generation, he has been the dean of Swedish historians in the United States.

Scott's purpose is to explain how this small nation of about eight million, underdeveloped as recently as a century ago . . . "has emerged in the twentieth century as a major exemplar of peaceful neutrality, of the 'Middle Way' and the welfare state, as a disburser of the Nobel Prizes, the homeland of world-renowned scientists, writers, actors, and artists, with a technological culture second to none." His method is "to give a cohesive explanation of national development . . . frankly

based on the concept that the nation as a community of people has forged the most constructive bands of large-scale cooperation among men." While he admires the way in which modern Swedes go about trying to solve their problems (if not all of their solutions), "... the record must speak for itself and that is what this book is all about—not in a spirit of praise or blame or justification but as an attempt to delineate the course of a unique national history."

The organization is broadly chronological, beginning with the last Ice Age and ending with 1976, with topical chapters and subsections interspersed throughout. While domestic politics and foreign policy are the guidelines, the economic, social, and cultural life of each period are in no way slighted. Indeed, one strength of this book is its length—nothing important is glossed over and there is space remaining for all manner of interesting side-lights and detail not found in other English-language histories of Sweden. One instance of this is the two-page discussion of Rutger Maclean, the large landowner in southern Sweden who did so much to promote the enclosure movement around 1800. Another example is the page and a half devoted to the fortunes of the Åtvidaberg industries over the centuries in south-central Sweden. The Maclean and Åtvidaberg stories intimately illustrate the revolutions in agriculture and industry.

There are many features which make Franklin Scott's book the finest history of Sweden available in English. First, he writes well. And, in dealing with fundamental issues, he clearly poses the questions raised and answers them with the best scholarly evidence available. He indicates the significance of complicated issues and periods once he has delineated and summarized them. Eighteen pages of notes, by chapter, provide the reader with Scott's sources and further elucidate material in the text. The "Selected Bibliography," also by chapter, is a listing of the standard and most up-to-date works in both Swedish and English, with emphasis on the latter. A list of predominately English-language periodicals is useful.

The text is sprinkled with short translated quotations from primary sources. While there are no passages in Swedish, Scott goes out of his way to teach the reader key Swedish terms. Another attractive feature is his frequent use of Swedish scholars in the body of the text when interpretation is called for; one is not left with the impression that the nation's history is all cut and dried.

The maps are adequate, but a second edition should include one of the current administrative districts (*län*). In addition, the map of northern rivers should label all of the rivers. A second edition might also have more complete genealogical

charts—although the author explains how rulers are related in his text. Every major political figure of the twentieth century is mentioned, generally with an insightful comment, with the exception of Prime Minister Tage Erlander. But these are small things.

The final two chapters deal with the welfare state and "The Modern Multifaceted Culture, c. 1875–1975," and it is here that the author harmonizes the seeming incongruities of contemporary Sweden. Scott points out that the problems facing modern Sweden are not unique to that nation but that "... the forthrightness with which Sweden faced these problems and the forcefulness of the role played by government" is unique (p. 544). The final chapter covers everything from modern arts and crafts to religion and concludes with his estimate of the characteristics of twentieth-century Swedish society. According to Scott, democracy is most basic, followed by nationalism, and a "striving for excellence and demand for quality" (p. 583). Finally, the Swedes regard government as their agent, "... created to do their will" (p. 584). This history illustrates that some peoples *do* profit from their experience.

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MICHAEL JONES. *Finland: Daughter of the Sea*. (Studies in Historical Geography.) Hamden, Conn.: Archon Books. 1977. Pp. 247. \$15.00.

The romantic-sounding title of Michael Jones' book is borrowed from the writings of Z. Topelius, better known today as a poet than for his role in the early nineteenth century as one of the first geographers to study the phenomenon of land uplift. Land uplift occurs primarily in northern latitudes, in part as a result of the retreat of continental ice sheets of the last Ice Age. Owing to the flatness of the terrain, Finland's area increases every hundred years by about a thousand square kilometers, most of it through land emergence along the western coast. Meadows emerge from the sea, ports and rivers disappear, villages are cut off from their traditional fishing grounds, and disputes abound.

In his study, Jones succeeds in presenting an engrossing, readable synthesis of all the major impacts of land uplift upon life in Finland. An initial chapter provides a modern explanation of the physical phenomenon, as well as a history of the debates which raged until the mid-1800s over its origins. The "Neptunists" maintained that the amount of water in the seas was diminishing while the "Vulcanologists" held to the theory of land upheaval.

A chapter on the human problem provides a brief historical survey of the impact of uplift. Much

of it is well known, such as the way in which the pattern of human settlement in Finland and Scandinavia changed in the Stone Age, Bronze Age, and Iron Age; modern archeological finds can frequently be linked directly to the presumed shifts in the coastline. For those concerned with historical development rather than geography, Jones' major contribution will be found in his generalizations about the principles underlying the human response to the problems posed by geographic change: what to do when the course of a river suddenly shifts, how to divide up hundreds of hectares of meadowland between members of a village, how to organize access to an "island" owned by one village in the midst of a "bay" belonging to another after the bay has completely dried up.

This leads to a complete review of the evolution of land tenure principles over the centuries. In this respect, events in Finland closely parallel those of other countries where partition of communal agricultural and forest lands was carried out as part of the process of modernization of agriculture. (Partition was initiated, of course, under Swedish rule and law.) However, unlike most other countries outside Scandinavia, the scope of the land uplift problem and the density of the population affected have caused Finland to make regular adjustments in law and practice right down to the present.

The topic is well worth the analysis, for it provides an interesting perspective on numerous problems. For example, one can trace the interaction between the conservatism of rural populations, and "progressive" legislation adopted over the centuries in response to changing conditions and pressures. The original impetus for laws to partition village lands in the eighteenth century was the desire to ensure efficient land use by elimination of the division of fields into strips (Swedish: *tegskifte*; Finnish: *sarkajako*); resistance came from those attached to this collective form of tenure, which obliged all in the village to work their lands together. Following partition into individual, unified plots under one owner, successive divisions of family holdings through inheritance resulted in a different pattern of fragmentation; new laws in the nineteenth century sought to rationalize these holdings but met with resistance in the name of private property. In the twentieth century, increasing use of shore lands for recreational rather than agricultural purposes has led to further refinements of law and practice.

Throughout his work, Jones demonstrates excellent scholarship and writes with clarity for the non-technician. The book includes an abundance of maps, sketches, and drawings, obviously produced with a rare attention to detail.

STACY CHURCHILL

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MICHAEL G. BAYLOR. *Action and Person: Conscience in Late Scholasticism and the Young Luther*. (Studies in Medieval and Reformation Thought, number 20.) Leiden: E. J. Brill. 1977. Pp. x, 288. f 68.

Luther ended his famous refusal to recant at the Diet of Worms with the words, "It is neither safe nor right to go against conscience." What did he mean by conscience? Naturally, he meant what his teachers had meant. We have, then, a lengthy account of previous concepts, briefly those of Paul, Jerome, Augustine, and Abelard and then much more extensively those of the *Via Antiqua*, represented by Aquinas, and the *Via Moderna* as exemplified in the thought of Ockham and Biel. These two schools of late scholasticism differed but little on the subject of conscience.

For them conscience was the faculty innate in man, called the *synteresis*, for making ethical decisions with respect to specific acts. These decisions were made by rational processes based on knowledge. Sometimes *conscientia* was equated with *conscientia*. Conscience was a firm conviction of the rightness of a course based on this information. If the information was incorrect the conviction was none the less binding, provided the delusion was due to "invincible" ignorance, that is ignorance which could not have been corrected by fuller examination. Conscience was directed to specific acts.

Luther was at first of this opinion, but in time he came to realize that in God's eyes every act of man is sinful. No *synteresis* is necessary to prove that, and the concept is dropped. Conscience is the recognition of the human condition, of the state of the whole person rather than specific acts. It is not a rational deduction from information. The only way out is not human rectitude but divine forgiveness.

Michael G. Baylor carries this analysis through in great detail and with meticulous critical analysis. Those with whom the author disagrees, as he does at times with me, will need to re-examine their positions. There is, however, one point of significance to which he barely alludes, namely Luther's recognition that the "saints" have bad consciences, while abandoned scoundrels have easy consciences. David, when he had contrived the death of Uriah and had taken to himself his wife, was not troubled until the prophet confronted him with "Thou art the man." And Jonah, having disobeyed the command of the Lord to preach repentance to the people of Nineveh and having taken ship for Tarsish in a raging storm, was asleep in the hold of the ship and would have forgotten his disobedience had not the cast of the lot disclosed him as the cause of the tempest. Luther rendered the phrase in the psalm "secret sins"

by "unrecognized sins." No wonder Luther would drop *synteresis*, if that is all the good it does!

Then came for Luther all the practical questions. All men are sinners, but they are not all criminals. In the eyes of men there are gradations. Had the peasants a right to rebel? If the emperor tried by arms to suppress Lutheranism should he be resisted by arms? Is war against the Turks legitimate? Should town councils forbid begging? May Anabaptists be executed? One would like to know how conscience figured in all of this. One suspects that Luther would be back on scholastic ground. Another book is in order. Baylor's conclusion is that Luther heralded the modern world by liberating conscience from the authority of the Church. Right!

ROLAND H. BAINTON
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SIGRID JAHNS. *Frankfurt, Reformation und Schmalkaldischer Bund: Die Reformations-, Reichs-, und Bündnispolitik der Reichsstadt Frankfurt am Main, 1525-1536.* (Studien zur Frankfurter Geschichte, number 9.) Frankfurt am Main: Verlag Waldemar Kramer. 1976. Pp. 443.

Sigrid Jahns accepted Bernd Moeller's invitation for renewed investigation into the phenomenon of "*Reichsstadt und Reformation*." In a 1972 doctoral dissertation, she attempted to present a comprehensive history of Frankfurt's political bearing with regard to the Reformation, the emperor and empire, and evangelical leagues. The well-written result has now appeared without substantial changes, and it largely attains the author's goal.

The appearance of the Reformation caused a crisis for Frankfurt's patrician government because the city had received numerous privileges from the emperor. Among these were the rights to hold semiannual fairs and to host elections of emperors and kings, that is, rights upon which the patriciate based much of its wealth. If the government formally accepted the Reformation, the emperor might revoke these privileges; if it rejected the Reformation, the government risked a rebellion from its populace such as the one that occurred in 1525. By 1533 when the administration officially introduced the Reformation, it had only partially avoided this crisis while it had successfully isolated itself not only from a large number of Frankfurt's people but also from Catholic and Protestant powers—the emperor, king, and elector-archbishop of Mainz who was the city's ordinary, on the one hand, and electoral Saxony, Hesse, and the Schmalkaldic League on the other.

Much of this has been told before. Jahns, however, offers new insights, especially in the complex

legal sphere. In her discussion of the processes lodged with the imperial supreme court, for example, she shows that Albrecht of Mainz, a man who favored a moderation of the religious question in the empire as a whole, influenced the emperor, king, Catholic electors, and court to act against Frankfurt because of its evangelical views. The imperial *fiscal* and Mainz's cathedral chapter reinforced the archbishop's endeavors. Only after frenetic diplomacy at all levels and an adjustment of its religious views to accord with those of the *Augustana* was Frankfurt finally admitted to the Schmalkaldic League in 1536. This act stabilized both the government and the city's Reformation until the Schmalkaldic War.

Based extensively on unpublished archival materials in Frankfurt, Marburg, Strasbourg, Ulm, Nuremberg, and elsewhere, Jahns' book is well researched and compelling. Those readers interested in socioeconomic analyses might be disappointed, but all who are concerned with the Reformation in the cities will find this work valuable.

PHILLIP N. BEBB
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JOACHIM F. FOERSTER. *Kurfürst Ferdinand von Köln: Die Politik seiner Stifter in den Jahren 1634-1650.* (Schriftenreihe der Vereinigung zur Erforschung der Neueren Geschichte e. V., number 6.) Münster: Verlag Aschendorff. 1976. Pp. ix, 455. DM 118.

Thorough, solid, painstaking—Joachim F. Foerster's monograph is all of these things. A student of Max Braubach, Foerster presents here a detailed narrative of the diplomatic maneuvers of the Archbishop of Cologne as he tried to hold intact the ecclesiastical territories of Hildesheim, Liège, Münster, and Paderborn, as well as several smaller territories and the archiepiscopal lands themselves. Elector Ferdinand of Cologne (brother of Elector Maximilian of Bavaria) was a figure of some importance in the constant turmoil of the last years of the Thirty Years War, but until now no scholar had ever thought to devote a full study to him and his policies. Foerster decisively closes this gap in our knowledge by examining in succession the tricky negotiations to regain Hildesheim from the House of Braunschweig-Lüneburg, the military and diplomatic efforts to push the Hessian troops out of Westphalia, the efforts by the elector to reform the defenses of the Westphalian Circle, and the complex mass of proposals and compromises that finally brought peace in 1648.

In all of these dealings, Elector Ferdinand displayed one consistent characteristic: a steadfast refusal to compromise the Catholic faith. This intransigent position brought him into conflict with

the other Catholic electors (Mainz and Bavaria, especially) and, of course, with the emperor as well. In his staunch fidelity to an uncompromising Catholicism, Ferdinand was clearly unusual—and perhaps unique—among the princes of his day. Unfortunately, Foerster does not even try to explain the sources of Ferdinand's attitude. This is, after all, not biography, but diplomatic history in the strictest sense; it appears as volume six in a monograph series devoted entirely to the Peace of Westphalia. Scholars working closely on this treaty will find Foerster's meticulous work useful and massively documented (2,241 footnotes). Other scholars will regret that Foerster so rarely raises his eyes from the documents to reflect generally on the significance of the events which he recounts.

H. C. ERIK MIDELFORT
University of Virginia

ROLAND-GÖTZ FOERSTER. *Herrschaftsverständnis und Regierungsstruktur Brandenburg-Ansbach, 1648-1703: Ein Beitrag zur Geschichte des Territorialstaates im Zeitalter des Absolutismus*. (Mittelfränkische Studien, number 2.) Ansbach: Historischen Verein für Mittelfranken. 1975. Pp. xiii, 317.

Absolutism—or more properly speaking, the expansion of central administration in the states of early modern Europe—provides the impetus for this study. Through a detailed analysis of royal personalities and political institutions in the Margrave of Brandenburg-Ansbach, Roland-Götz Foerster maintains that the process of centralization, at least in the context of the smaller principalities, resulted more from practical necessity than from deliberate bureaucratic implementation of political theory or from aggressive princely initiative. He carefully examines the ordinances and testaments of the princes and looks more generally at the workings of the principal committees of government, concluding that, although their behavior came increasingly to reflect “absolutist” priorities, rulers and bureaucrats alike continued to think of themselves in traditional categories of Lutheran piety and polity. The demands of almost a century of warfare rather than conscious reflection, he maintains, produced new emphases within the government, shifts that were effected more through complicated litigation than by brute force and that took place with the tacit consent of all parties concerned rather than as a result of revolutions from above. With this stress on practical necessity rather than on sociological imperative, Foerster suggests a different emphasis from that of the essays in Charles Tilly, ed., *The Formation of National States in Western Europe* (1975) and follows

more in the tradition of scholars like G. Oestreich, H. H. Hofmann, and F. Wagner. Indeed, it was under Wagner's supervision that this work was first completed as a Ph.D. dissertation for the Ludwig-Maximilian University at Munich.

Foerster sees the local study as offering the most rewarding way in which to investigate European absolutism, and in this regard he may indeed be correct. But the work suffers from a failure to define precisely what is meant by absolutism—in some instances he seems to mean rationalized administration; in others the whims of a selfish, capricious prince—and from an apparent inability to differentiate between those elements of a purely local or antiquarian interest and those relating to the larger issue under discussion. Without some sort of comparative perspective, it is difficult for the reader to know just how much analytic importance to assign to discoveries such as the fact that a seat on the Ansbach *Hof Rat* usually signified membership in the Privy Council or that the Ansbach estates never developed a political initiative.

JAMES ALLEN VANN
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GISELA LANGE. *Das ländliche Gewerbe in der Grafschaft Mark am Vorabend der Industrialisierung*. (Schriften zur rheinisch-westfälischen Wirtschaftsgeschichte, number 29.) Cologne: Rheinisch-Westfälisches Wirtschaftsarchiv zu Köln e. V. 1976. Pp. 255. DM 29.80.

A substantial number of recent studies have documented the importance of dispersed handicraft production for later industrialization. Particularly important in the German case is the County of Mark, a well-known center of textile and metalware production in the eighteenth century which fused into the emerging Rhenish-Westphalian industrial region during the nineteenth century. In this study, although Gisela Lange presents a large body of information, the conceptual and organizational weaknesses will force readers to extract useful material for themselves.

Lange imposes a functional division on the subjects of the study, dividing them into “workers” and “entrepreneurs,” but the definitions are inappropriate to eighteenth-century reality. Most “workers” prove to be landholding peasants as well as artisans, not an industrial labor force. Many “entrepreneurs” are in fact small artisan masters—neither Marxian capitalists nor Schumpeterian innovators. Contemporary statisticians were actually more accurate when they occasionally listed all “workers” in a branch of production as “entrepreneurs” (pp. 35-36).

Lange asserts at every opportunity that rural artisan manufacture provided a basis for later "industrialization," but does not show the mechanism of the transformation or even offer a definition of industrialization. Worse is the conclusion that because artisan manufacture was widespread, "industrialization" must have been an evolutionary and not a revolutionary process. This ignores the revolution in the technical process of production, which was no less revolutionary for having been introduced on occasion by previously existing firms (p. 188). It also obscures the revolution in the social organization of production. The artificial division of eighteenth-century communities into "workers" and "entrepreneurs" will not permit study of the process by which some eighteenth-century merchants and peasant-artisans became nineteenth-century industrial entrepreneurs, while the remainder (and their children) developed into an industrial labor force—a process under way in Mark by the 1760s, though still in its early stages.

Crucial questions therefore remain unanswered, and Lange's limited theoretical perspective and relentless provincialism pose other problems as well. For instance, on the evidence Lange presents, eighteenth-century Mark could be described as a technologically backward district (p. 148) dominated by restrictive cartels (pp. 113 ff.), with no improved highways (p. 155), a grossly inadequate educational system (pp. 29, 32–34), and declining real income pressing on the bare subsistence level by the 1780s (pp. 28, 186–87). But of course it was one of the most advanced districts on the Continent—the paradox arises only from Lange's failure to set Mark in a comparative perspective. Material from neighboring districts and from other industrial areas (especially France and England) should have been employed. Organizing such material would have required more extensive and systematic use of regional economic models, but would have made it possible to address such questions as Mark's relative degree of development, variations in organization, and the relative impact of economic and political influences in creating this important pocket of proto-industrial development.

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MANFRED BOTZENHART. *Deutscher Parlamentarismus in der Revolutionszeit, 1848–1850*. (Handbuch der Geschichte des deutschen Parlamentarismus.) Düsseldorf: Droste Verlag. 1977. Pp. 886. DM 160.

The notable German commission for the history of parliamentarism and the political parties is publishing a *Handbuch* of the history of German parliamentarism in sixteen volumes covering the period

from the beginning of the nineteenth century to the present, of which Manfred Botzenhart's book on the 1848 revolutions is the first to appear. So far the commission has been responsible for detailed, well-defined studies in certain aspects of its sphere of interest, which have proved of sterling worth to students of German history. With the *Handbuch* the commission enters a field in which delimitation is far more difficult. It is impossible to isolate the whole concept of parliamentarism and its actual expression in assemblies from the problems these institutions were summoned to try and solve.

The author appreciates that in historiography theoretical discussion of the parliamentary system has hitherto been somewhat overshadowed by the question of the foundation of a united German state on a liberal basis, though he is not quite consistent in applying this insight in his book. In order to avoid sinking in a morass of detail, Botzenhart realizes the need to stop short of a history of parliaments, but to some extent he has tried to do once more the work which has already been done by specialists in various segments of the history he covers. The result is a text of nearly eight hundred pages, packed with facts, which makes considerable demands on the reader, as the author does not always succeed in pulling together the threads of the story by analysis. He has done an enormous amount of reading, his memory is prodigious, and when he does analyze, the standard is high. Much of the information he gives is of great interest for the study of parliamentarism. He brings out the lack of theoretical preparation regarding the nature and mechanics of parliamentarism in the pre-1848 period, the regional variety of a multitude of relationships between government and opposition both before 1848 and in the 1848–50 period, the interesting impact government office made both on moderate liberal and somewhat more radical politicians, and the complexity of the issues which governments and chambers were called upon to solve.

The hard core of the book is the development of the parliamentary idea in the 1848–50 period. Botzenhart succinctly examines the understanding that various groups, including rulers and members of royal houses, as well as numerous shades of moderate liberals and radicals, showed of parliamentarism, and how they modified their ideas under the pressure of events. With great ability he tests these ideas by their application to certain constitutional issues, such as the ruler's absolute or suspensive veto and the degree of his influence on the appointment of ministers. Substantial evidence is produced to indicate a considerable degree of moderate liberal support for a restricted indirect franchise, based on a somewhat touching belief—at the outset of the revolutionary period—

in the political judgment of a broad *Mittelstand*, reaching down to the propertied lower middle class. This approach was not quite so exclusive as it appears because of the possibility—at any rate in theory—of the disfranchised being able to rise to the enfranchised classes.

The author rightly points out the reluctance of some moderate liberal governments in the revolutionary period to submit themselves to the verdict of the electorate, in the expectation that they would lose out to the more radical parties. But in his judgment he is charitable to the moderate liberals, defending them against excessive condemnation. Botzenhart gives a remarkable dossier of divisions within the groups to the left of the moderate liberals, and relates their parliamentary groups in the various assemblies to the political associations (*Vereine*) which flourished at the time, thus enriching our knowledge of the formation of opinion in this period. By careful analysis of such material as membership and voting figures he shows the limitations of the strength of the Left. He does not hide the squabbles which took place in their ranks, but as—with the moderate liberals—comes to a balanced judgment. The Left, whatever poor political judgment it may have shown at times during the revolutionary period, remained more loyal to its ideals than did the liberals.

In spite of the criticisms which have been made, the book will find its place in the literature of the period and clearly establishes Manfred Botzenhart's scholarly reputation.

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KARL HEINZ BÖRNER. *Die Krise der preussischen Monarchie von 1858 bis 1862*. (Akademie der Wissenschaften der DDR, Schriften des Zentralinstituts für Geschichte, number 49.) Berlin: Akademie-Verlag. 1976. Pp. 246. 26 M.

Karl Heinz Börner, working under the tutelage of Rolf Weber at the East German Central Institute for History, has produced a solid and orthodox Marxist monograph on the crucial period of Prussian history between the accession of William I as Prussian monarch and the naming of Bismarck as minister president. Systematically, he discusses the economic and political bases of the Prussian monarchy, the "New Era," the military reform, the opposition of the House of Deputies, the constitutional conflict, and Bismarck's appointment.

He bases his interpretations firmly on the standard East German work of Ernst Engelberg. Struggles at court between the ultraconservatives who ruled under Frederick William IV and Wil-

liam's "New Era" moderates were only differences within the "ruling class over the most workable methods to secure the existing power relationships" (p. 31). The bourgeoisie had failed in its historical role in 1848 and had chosen a course lacking in courage; its unwillingness to make a common front with revolutionary democrats against the monarchy led to a development of a "bourgeois German national state which was contrary to the objective interests of the German people" (p. 38). The only interpretation surprising to this reviewer is Börner's defense of William's army reform as one corresponding to the "objective needs" of the "new economic, social, and military conditions" (p. 88).

Börner is thoroughly familiar with the printed German-language primary and secondary sources, whether pre-1945, East German, or West German. He has not used non-German works, however, thus losing the perspectives offered by such American historians as Hamerow, Krieger, and Pflanze. (Craig he uses in translation.) He has made excellent use of the files of the Prussian ministry and the crown council at the Central State Archives at Merseburg. He has used the papers of prominent conservatives and court figures, but he has not paid equal attention to the papers of the liberals. He neither undertakes an analysis of the revolutionary democratic forces in Prussia at the time nor attempts to assess their strength. The prose of the monograph is regularly spiced with the ironic comments on Prussian affairs of Marx and Engels. Technically, the book is well printed, though cheaply bound in paper.

Specialists in the period will be grateful for Börner's workmanlike effort. It adds to our store of information on the day-to-day political events at the Prussian court during the formative years of William's monarchy, while spelling out in detail a Marxist view of that subject.

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HELMUT HIRSCH. *Freiheitsliebende Rheinländer: Neue Beiträge zur deutschen Sozialgeschichte*. Düsseldorf: Econ Verlag. 1977. Pp. 272. DM 28.

This volume consists of ten previously published essays, each of which treats a Rhinelander active in the nineteenth century as a critic of the political and social status quo. Despite their billing as contributions to social history, for the most part they ought to be regarded as studies of intellectual history and of the biographical background to social thought. The essays vary considerably in aim and scope. Three of them, originally written for a collection of biographies of residents of Cologne, pro-

vide general overviews of the life and thought of Robert Blum, Moses Hess, and August Bebel. At the other extreme, one essay on Karl Marx treats his views of Napoleon Bonaparte, another emphasizes his efforts in 1843 to advance the cause of Jewish emancipation, and a third focuses on the sympathies of Friedrich Albert Lange for the North in the American Civil War. Other pieces treat various aspects of the careers of Friedrich Engels, Carl Heinrich Marx, Ferdinand Lassalle, and Karl Ludwig Bernays.

The essays reveal considerable learning, display a lively style, and clearly convey Helmut Hirsch's warm sympathies for the men about whom he writes. Except, however, for those that are quite narrowly conceived and thus of limited interest, they generally lack clear points of focus and tend to dissolve into impressionistic collections of anecdotes. The lack of unity and direction evident in many of the individual pieces is all the more striking when one considers the volume as a whole. Granted that this defect is to some extent inherent in the genre to which the book belongs, the failure to assess these men collectively and comparatively as examples of a type—or types—of radical thinkers is still glaring. One would also like to find some general remarks about the specifically Rhenish dimensions of their careers, as well as an attempt to contrast them with the more moderate Rhinelanders who did so much to further the cause of liberalism in the 1840s. As it stands, the book makes marginal contributions to our understanding of themes that deserve more systematic attention than they receive here.

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STEPHEN M. POPPEL. *Zionism in Germany, 1897–1933: The Shaping of a Jewish Identity*. Philadelphia: Jewish Publication Society of America. 1977. Pp. xviii, 234. \$7.95.

Recent books on the question of German-Jewish self-identity have concentrated on the conflict between the assimilationist majority and Zionist minority. Stephen M. Poppel's provocative, well-conceived work approaches the problem differently by focusing on the Zionist movement in its Jewish, German, and world contexts. With minimal attention to institutional development, Poppel turns to what really interests him—Zionism's appeal for some German Jews as revealed in the periodicals, memoirs, and private papers of the movement, and in the behavior of individuals.

This choice of emphasis is a reasonable one because, judged by almost any standard, the Ger-

man Zionist movement was a failure. It did not win away many converts from "degrading" assimilationism. Worse, the Zionists seemed unable to take their own program seriously. Even though eventual *aliyah* (emigration) had been adopted as an obligation in 1912, only two thousand German Jews went to Palestine between 1920 and 1932. Further, those Jews who did emigrate were notoriously slow in giving up their German lifestyle; they clung to the language, dress, and occupations they had practiced in Germany instead of learning Hebrew and "returning to the soil."

Noting this gap between Zionist rhetoric and performance, Poppel formulates his central question. If it was not an overwhelming success among German Jews or in the arena of international Zionist politics, what function did Zionism perform for its adherents? His main answer, which will not satisfy everyone and may even offend some, has to do with the inescapable Germanness of German Jews. The deep-seated attachment to German culture has long been obvious among assimilationists, but Poppel shows that the Zionists were just as fully acculturated. For both Zionist and assimilationist the existence of political anti-Semitism and the rejection of full Jewish integration that it implied was severely disturbing. The assimilationists' response was to minimize "Jewish cultural peculiarities" and to reduce Judaism to a religious affiliation. Thus they sought acceptance as "German citizens of Jewish faith." Zionists found this craven. They thought that by proudly accepting, even emphasizing, their Jewish nationality it would be possible to disarm the anti-Semites who insisted on the exclusion of Jews from German life. However, the espousal of Jewish nationality, while utterly sincere, did not require and could not accomplish the renunciation of German culture, those formative influences of environment, class attitude, and education. Zionists accepted *eventual aliyah* but in the meantime they, no less than the assimilationists, sought a viable means of remaining in Germany. Both Zionists and assimilationists predicated their "solutions to the Jewish question" on the existence of an imaginary German tolerance for pluralism. Both sought, and were apparently satisfied with, their own answers to the perplexing indignities of anti-Semitism. Despite their heated public debates with one another, they were really very much alike.

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GERHARD A. RITTER. *Arbeiterbewegung, Parteien und Parlamentarismus: Aufsätze zur deutschen Sozial- und Verfassungsgeschichte des 19. und 20. Jahrhunderts*. (Kri-

tische Studien zur Geschichtswissenschaft, number 23.) Göttingen: Vandenhoeck and Ruprecht. 1976. Pp. 412. DM 64.

With exception of the brilliant composition on the transformation of the trade unions, the ten essays in this felicitous collection focus on one theme: the obstacles and structural inadequacies that prevented the emergence of responsible parliamentary government in either imperial or Weimar times. Stressing the historic imbalance between political and economic power in Germany, Gerhard A. Ritter, an outstanding historian, makes incisive comparisons between the English and German models to illustrate in richly footnoted essays the deviation of the German system from the West European norm. The critically defective German structure featured denatured political parties (more identified with economic interest groups and classes than with policies) and their dualist separation from government. While it is true the founders of the Weimar Republic misunderstood the workings of the parliamentary system, it is a fact that party personnel and purposes—largely transmitted from the Empire—did not conform with that system. Even in the *Bundesrepublik* social and political obstacles still bar the way to a healthy polity, and power vacuums continue to invite dangerous adventures by small groups. The structural defects of government in the BRD as much endanger freedom today as did misuse of state power in the past, and the egregious decline of the power of parliament vis-à-vis the executive is a cardinal cause of the widespread uneasiness that currently afflicts the friends of political democracy.

Most of Ritter's valuable insights into the laming influences upon the German parliamentary system are not exactly original. In regard to the Second Reich, especially, they stem from other historians—Puhle, Nipperdey, Kaelble, Stegmann, Groh, Witt, Stürmer, etc. However, their conclusions have been skillfully woven into this gravamen against Germany's baleful authoritarian legacy. Ritter's judgments support the "continuity thesis" of the Fritz Fischer "new orthodoxy"—in this case, the continuity of character and personnel of political parties from the Second Reich through the Weimar Republic. Notwithstanding some contradiction, Ritter, in accord with Boldt, Witt, and Stürmer, avers that the imperial governmental structure was neither autochthonous nor enchorial but generic and transitional. On the other hand, he rejects Böckenförde's view that the German polity was predestined to progress from *Obrigkeitsstaat* to parliamentary democracy. Here it is legitimate to ask whether, considering the reformist impact of the practical activity of the trade unions and Social Democracy, we absolutely must em-

brace as apodictic Ritter's conclusion that the Reich parliamentary model did not possess the capability of evolving into more than a simulacrum of the English or French. It should also be asked whether German parliamentary beginnings were not stultified principally by the prestige and power of the grand military and diplomatic victories of Prussia had conferred upon the executive and his creatures.

WILLIAM H. MAEHL
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URSULA MITTMANN. *Fraktion und Partei: Ein Vergleich von Zentrum und Sozialdemokratie im Kaiserreich*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 59.) Düsseldorf: Droste Verlag. 1976. Pp. 455. DM 72.

By the first years of this century it was evident that the Social Democratic and Center Parties, both representative of depressed groups, would share most of the popular vote in future German elections. The two parties were radically different in organization and in their perceptions of the roles their members or electors should play in the formulation of policies. The author of this excellent study concludes, however, that circumstances proved stronger than men and marked similarities appeared in the functioning of the two parties before and during World War I.

In its formal structure the SPD was a model of political democracy: the Reichstag fraction was subject to the authority of the national council, and the acts of both required approval from the annual party congress. The Center, like other middle-class parties, was a parliamentary fraction led by dignitaries rather than a national party based on a democratic foundation. Ursula Mittmann may not stress adequately enough the difficulties the leaders experienced in winning broad Catholic support for a government which discriminated against their co-religionists; nevertheless, it is revealing of the leaders' authoritative position in Centrist politics that they could always count on enthusiastic acclamation from the annual Catholic Congress which dealt indiscriminately with religious and political matters.

Mittmann notes a growing convergence, however, in both parties' manner of functioning at least a decade before the outbreak of the First World War. Democracy and mass organization were seemingly incompatible with each other. As Robert Michels had already noted in 1906, the growth of the Social Democratic movement had led to oligarchical rule in the party. August Bebel and Paul Singer were no more eager than the heads of the Center fraction—or, one might add,

the distinguished Liberal leader, Eugen Richter—to make their party press privy to the secret deliberations of the leadership of the parliamentary group. In 1914 pragmatism, patriotism, and the pressure of circumstances led the Center fraction to approve without debate the government's declarations of war and the invasion of Belgium. Though there was some debate in the Social Democratic fraction over those issues, that body and the General Commission of the Social Democratic trade unions—acting completely on its own—decided to follow the same course for essentially the same reasons. The SPD fraction majority tolerated the monarchy's annexationist policy, thus causing the tragic schism within the Social Democratic Party before the leaders acted as co-sponsors with the Center and Liberals of the peace resolution of July 1917. The efforts of the Center fraction to abandon its own earlier annexationist orientation and secure broad Catholic backing for the peace resolution almost produced a similar split in the wider Center Party.

In her summary the author concludes that the Centrist leaders at all levels were not interested in democratizing their own party and were honorable enough to state that democracy was not compatible with the aims and needs of their constituency. She adds that while the Social Democrats had failed to bring their party members into the discussion and formulation of policies, they did not give up their attempts to do so. Most historians will share the author's appreciation of that moral effort; some will continue to ask the question whether the party leadership might not have profited from the Center's pragmatism in other respects.

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HEINER RAULFF. *Zwischen Machtpolitik und Imperialismus: Die deutsche Frankreichpolitik, 1904/06*. Düsseldorf: Droste Verlag. 1976. Pp. 215. DM 39.

The importance of this doctoral dissertation lies not in the new material that it contains, but in its approach. Heiner Raulff has attempted to integrate the two, often antagonistic, treatments of German foreign policy by interweaving the roles played by domestic affairs with a detailed investigation of the formulation and execution of foreign policy. Rather than following the chronological method most often used by diplomatic historians, each of the author's chapters discusses another aspect of the period which influenced the shaping of events. This structural approach has the distinct advantage of demonstrating how the personalities,

rivalry, influence, and views of France of the principal formulators of German policy, the Russo-Japanese war, the rivalry between the naval and military high commands, economic interests, and the opinions of press and influential politicians all interacted to produce a series of events the aims and goals of which have been the source of much controversy.

Briefly, the conclusions he reaches are that Franco-German relations were not central in the thinking of the formulators of policy, but tangential to Germany's relations with England and with Russia. It was not until after Germany's overture of an alliance had been rejected by Russia and the latter had suffered serious defeats by Japan that the March 1905 decision to pursue an active policy in Morocco was made. The Russian refusal of an alliance solidified the two factions in the foreign office—one, led by Holstein, favoring a policy of European power-politics, and the other, led by Richthofen, demanding the pursuit of imperialism. All now agreed that France, especially Foreign Minister Delcassé, was the source of Germany's troubles. Until Delcassé's resignation in June, Holstein dominated the scene and directed the actions that led to an escalation on the crisis; German policy, therefore, was controlled by its concern for Germany's role as a European great power and for Germany's prestige. After July 8th and the conclusion of the Franco-German agreement for a conference, the unanimity in the foreign office dissolved and the influence of the imperialist group increased.

During the March to July period Raulff believes that the ruling circles in Germany actively debated the question of war with France. Schlieffen was in favor and Bülow had practically abdicated his role by handing over the direction of German policy toward France to Holstein. The decision for war or peace lay not in Berlin, but in Paris between Rouvier and Delcassé.

From the point of view of the ruling group, economic interests were never central either in influencing German policy nor as its goal, and it is a mistake to view German attitudes toward Turkey and Morocco as two aspects of a common policy. It was overwhelmingly a policy dominated by power politics and supported by a wide spectrum of political opinion—not opposed even by Bebel—that was responsible for the first Moroccan crisis.

The volume is successful in weaving together the multiple strands that produced the final tapestry, and demonstrates the complex background against which policy decisions of any kind—foreign and domestic—are made. More such attempts would most probably prove equally rewarding. One note of criticism, however: the work would have been improved by the addition of a

conclusion summarizing the author's interpretations.

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KURT DOSS. *Das deutsche Auswärtige Amt im Übergang vom Kaiserreich zur Weimarer Republik: Die Schülersche Reform*. Düsseldorf: Droste Verlag. 1977. Pp. 328. DM 58.

This careful study deals with a crucial phase in the history of the German foreign service, the period of transition from Empire to Republic at the end of the First World War and the reforms initiated at that time under the direction of Edmund Schüler, an official of the *Auswärtiges Amt*. Although the book was written as a dissertation, it is not the work of a beginner. The author, in his early seventies, can look back on a long and fruitful career in German secondary education, and his age and experience are reflected in the judicious approach and mellow tone of the book.

The "Schüler reform" called for the reorganization of the German foreign service along regional rather than functional lines; it merged the hitherto separate careers of political and consular officials; it opened the door to talented "outsiders"; and most important, it advocated a greater influence for economic considerations and interests on German foreign policy. As an attempt to modernize the conduct of foreign affairs, these reforms were not unique to Germany, as has been shown in the recent study by P. G. Lauren, *Diplomats and Bureaucrats. The First Institutional Responses to Twentieth-Century Diplomacy in France and Germany* (1976), which covers some of the same ground.

The overdue reforms of the German foreign service, as Kurt Doss shows, were not the result of the German revolution of 1918-19, although they coincided with it. Demands for such change had already been made before 1914 and had gained momentum during the war. To place his story in a wider perspective, the author discusses, perhaps at too great length, Germany's wartime relations with Argentina, which had been strained by the sinking of Argentine ships and the ineptitude of Germany's minister to Buenos Aires, Count Karl Luxburg. It was the "Luxburg affair"—touched off by U.S. publication of some intercepted telegrams in which the count referred to Argentina's foreign minister as an "anglophile ass"—which led to protests from the Hanseatic cities afraid for their overseas trade. The leaders of Hamburg and Bremen were close allies of the reformers within the *Auswärtiges Amt*.

Schüler was not alone in his reform efforts. He was supported by some of his colleagues and by

the foreign ministers of the time, Count Ulrich von Brockdorff-Rantzau and Hermann Müller. Since his proposed changes affected many vested interests in the German foreign service, however, he also ran into strong opposition from the conservative old guard, and some of Schüler's reforms, after his sudden and still not fully explained resignation in December 1920, were ignored or watered down.

This meticulously researched book is based on the archives of the German foreign ministry and the city of Hamburg. The author was denied access to the foreign ministry's personnel files which, contrary to the spirit of the agreement of 1956 covering the return of the captured German documents, remain closed to scholars. Use of this source would have made possible a more thorough structural analysis of the German foreign service. But even with this limitation, this is a good book and a valuable contribution to the history of the German foreign office.

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HARRY F. YOUNG. *Prince Lichnowsky and the Great War*. Athens: University of Georgia Press. 1977. Pp. xii, 281. \$11.00.

If anyone had a chance, in the years before 1914, to mediate between the overly ambitious, overly nervous policy-makers in Berlin and what Harry F. Young rightly calls the "anti-German phalanx" of the British Foreign Office, it was Prince Lichnowsky. He made just about the ideal German ambassador to London. He was attractive, he was charming, he had intelligence and integrity, and he possessed the Kaiser's friendship and confidence. In his attitudes and in his policies, he was as liberal as a German aristocrat might safely be in either London or Berlin. "Never has a foreign ambassador achieved such rapid or resounding popularity," wrote Harold Nicolson, who did not gladly praise a German. Yet even Lichnowsky failed to prevent the disaster of 1914. In fact, the July crisis turned a happy man into a bitter one. Much of the rest of his life would be spent in sharp recriminations with various German government spokesmen about who should bear the blame for the decision to go to war. The debate strikes us as being not quite real today. Enough could be written about precisely what Grey had said, or not said, to Lichnowsky on August 1, 1914, for instance, for at least a master's thesis. Had hopes been held out for British neutrality; what offers had been made or withdrawn; just who had misunderstood whom? Yet in the confusion of these final days, how could there possibly have been no misconstructions?

The sad part was that Lichnowsky, so open-minded before, and so blessed with a sense of proportion, would develop something akin to a Kohlhaas complex. He had been wronged, he felt, abandoned by his friends, misunderstood by the Kaiser, and he would prove it, even if his world should perish! But then, how easy it is to be judicious sixty or more years later, how painless to say that both sides were right—Lichnowsky in pointing to the errors in German policy (from the first, he had been opposed to allowing Austria's Balkan schemes to involve Germany); his opponents in the German Foreign Ministry in considering him somewhat pro-British, and recalling Holstein's observation that "the average ambassador will prefer to go along with the government with which he is sojourning and against the government that is paying him."

At the time, the passions still had too much force, and it is Young's merit to make us see why this should have been so. This is a fine book, based to a large extent on archival material, and on conversations and correspondence with the Lichnowsky family. It contains no major new revelations. How could it? Barring a miracle, the time to expect these about 1914 is past. What it does is to give us a better, and fairer, picture of Lichnowsky than we have had, and that is quite enough.

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MARTIN KITCHEN. *The Silent Dictatorship: The Politics of the German High Command under Hindenburg and Ludendorff, 1916-1918*. New York: Holmes and Meier. 1976. Pp. 301. \$20.00.

Historians of Germany as well as military and diplomatic historians will welcome the publication of this fine study. Despite the self-evident importance of the political role of the military in Germany during World War I, the "dictatorship of Ludendorff" has not received the kind of objective and balanced attention it merits since the publication of Arthur Rosenberg's classic *Birth of the German Republic* in 1931. More modest in his goals than Rosenberg—whose scope extended to the entire gamut of wartime affairs—Martin Kitchen confines himself to a detailed treatment of the rising power of the Supreme Command under Hindenburg and Ludendorff between August 1916 and November 1918. Closely paralleling the work of such eminent historians as Rosenberg, Fritz Fischer, and Gerhard Ritter, Kitchen carefully documents the development of the German military dictatorship as the product of the Bonapartist, imperialist, militarist, and counter-revolutionary

tradition of the Prussian army that was facilitated by a four-year-long state of siege and the suspension of all domestic politics from 1914 to 1917 under the auspices of the *Burgfrieden*. World War I accordingly forms for Kitchen a transition between Bismarckian Bonapartism and the full-fledged, pseudo-democratic National Socialist regime of Hitler.

Undoubtedly Kitchen's major contribution is his detailed analysis—based on exhaustive documentary research—of the High Command's ever-growing power in such crucial matters as unrestricted submarine warfare, the July 1917 Crisis and the Reichstag Peace Resolution, the Treaties of Brest Litovsk and Bucharest, territorial arrangements in the Baltic and the Balkans, and finally, the conclusion of the armistice. Lending additional merit to this laudable study is Kitchen's provocative thesis that the Supreme Command carefully prepared the infamous stab-in-the-back legend as early as 1917 and that it was all prepared for dissemination in July 1918 when Ludendorff's spring offensive in France seemed to be grinding to an unsuccessful halt.

Unfortunately, this admirable work is not devoid of flaws. Kitchen fails to do full justice to the Supreme Command's struggle with the Reichstag, particularly during the chancellorships of Michaelis and Hertling. He virtually ignores the fight of the Left for parliamentary and franchise reform in the Reichstag and the Prussian Landtag. Finally, also missing are Kitchen's own provocative ideas about the connection between militarism and fascism as demonstrated by the views of Max Bauer (see *Central European History*, 8 [1976]: 199-220). All in all, however, Kitchen's *Silent Dictatorship* easily ranks as an extremely interesting and solid interpretation.

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EBERHARD KOLB and KLAUS SCHÖNHOFEN, editors. *Regionale und lokale Räteorganisationen in Württemberg, 1918/19*. (Quellen zur Geschichte der Rätebewegung in Deutschland, 1918/19, number 2.) Düsseldorf: Droste Verlag. 1976. Pp. lxxv, 504. DM 160.

Following the collapse of the monarchy in November 1918, soldiers' and workers' councils (*Räte*) arose, largely spontaneously, all over Germany. The councils shocked the bourgeoisie by their radicalism, but have been slighted or disparaged by historians for their ineffectiveness. Eberhard Kolb has, to borrow a phrase from E. P. Thompson, "rescued" the council movement "from the enormous condescension of posterity." Kolb first

dealt with the councils in a monograph, then published with Reinhard Rürup a documentary volume on the *Zentralrat*, and has now published with Klaus Schönhoven a documentary volume on the council movement in Württemberg; Rürup is preparing a companion volume for Baden.

Kolb and Schönhoven provide a balanced selection of documents, largely from the *Hauptstaatsarchiv* in Stuttgart. One could not ask for greater clarity of organization or greater meticulousness and utility of the critical apparatus (extensive footnotes, cross-indexing, a bibliography, a map, a thorough index, and a factual introduction to sketch in the historical background).

The war produced a quantum leap in the urban growth and industrial concentration of a formerly somewhat lagging Württemberg economy. The Württemberg Social Democratic Party, reflecting a long history of factionalism, had split apart in the winter of 1914–15, more than a year ahead of the split in other parts of Germany. Nevertheless, the documents show, the Majority Social Democrats never lost control of the Württemberg soldiers' and workers' councils to the Independent Socialists. Indeed, the Social Democratic cabinet of Wilhelm Blos, the Majority Social Democrats, and the councils worked closely together. To judge from the documents, the councils labored mightily and to a degree effectively in maintaining public order, demobilizing the troops, reorganizing the Württemberg army, overseeing the actions of local administrators, providing emergency housing, distributing food, and relieving unemployment. The eventual decline of the councils may be attributed to the solid opposition of the bourgeois parties (including the Democratic Party) and to the divergent trend of events at the national level. The councils had attempted to uncover hoarding, to control prices, and to distribute available housing according to need: did peasant and *Mittelstand* resentment against this interference with the rights of property condition the negative response of the bourgeois parties?

Carefully edited and handsomely printed, the volume will be a basic source for serious study of the early years of the Weimar Republic.

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JÜRGEN A. BACH. *Franz von Papen in der Weimarer Republik: Aktivitäten in Politik und Presse, 1918–1932*. Düsseldorf: Droste Verlag, 1977. Pp. ix, 354. DM 36.

For decades historians have studied Papen's chancellorship and role in the Nazi regime. Jürgen A.

Bach's book is unique in that it concentrates solely on the long neglected area of Papen's political activity before 1932. Bach thereby shows that Papen's policies as chancellor, as well as his collaboration with Hitler, were not merely responses to the crisis Germany faced after 1930, but attempts to fulfill goals which he consistently pursued throughout the Weimar era. Papen appears as an incorrigible antirepublican whose immutable perspectives on politics and society were conceived in the aristocratic-military tradition of Wilhelmine Germany. This anachronistic consciousness, reinforced by conservative Catholicism, inhibited any understanding of democratic-parliamentary ideals. Preoccupied with Christian conservatism and the Communist threat, Papen relentlessly sought to remold the Center into a rightist party, destroy the Prussian Weimar Coalition, and unite the conservative Christian forces in Germany against the SPD and Communists. Eventually these ideological predilections led Papen to believe that he could harness the conservative forces within the NSDAP in establishing an authoritarian Christian state.

Although this generally confirms the continuity in Papen's Weimar politics assumed by historians, Bach makes a substantial new contribution. He examines and documents in meticulous detail Papen's pursuits in the Prussian Landtag and struggle for control of the Center paper, *Germania*. Some of the most informative sections concern the dissension Papen caused in the Center, resulting in his isolation, and factional disputes over the nature of political Catholicism (in the context of the Weimar constitution). Quite revealing are the recently discovered papers of Count Hans Praschma, a Silesian Catholic associate of Papen. These documents prove that Papen, contrary to his memoirs and public statements, persisted in futile efforts to change the personnel and moderate editorial policy of *Germania* in order to reorient the paper, and ultimately the Center, toward the right.

The organization, editing, and printing of this book unfortunately resemble those of a dissertation. By dividing his study into two distinctly separate parts—Papen's political activity and the *Germania* affair—Bach disrupts the coherence of an otherwise readable account. Both sections, covering the same chronological period, should have been integral parts of a continuous narrative. An expanded discussion of the two crucial years preceding Papen's chancellorship, in lieu of the tedious financial details in the *Germania* controversy, would also have enhanced the value of the book. Certainly the events leading to the culmination of Papen's Weimar career deserve more than fourteen pages.

Still, specialists on Papen, the Center, and the

Weimar press will find this book quite valuable. Through exhaustive research into familiar and new documentation, Bach has illuminated an obscure, though decisive, segment of Papen's life. One should definitely consult this study to counterbalance Papen's palliative memoirs.

JOSEPH W. BENDERSKY
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BERND SÖSEMANN. *Das Ende der Weimarer Republik in der Kritik demokratischer Publizisten: Theodor Wolff, Ernst Feder, Julius Elbau, Leopold Schwarzschild*. (Abhandlungen und Materialien zur Publizistik, number 9.) Berlin: Colloquium Verlag. 1976. Pp. 251. DM 49.

A view current in liberal publishing circles in the later Weimar Republic was that the press had to fill the political vacuum resulting from the gradual elimination of parliament from the legislative process, in other words, that the press had to become in reality the power it was supposed to be in theory. Bernd Sösemann has taken four prominent Berlin journalists of a liberal-democratic stripe—Wolff and Feder of the *Berliner Tageblatt*, Elbau of the *Vossische Zeitung*, and Schwarzschild of the *Tage-Buch* and *Montag-Morgen*—and looked at their responses to the crisis-ridden last years of Weimar, in order to discover how responsibly they pursued this ideal view of the press.

Sösemann's research has broken some new ground. It is to his credit that he managed to ferret out, and succeeded in opening to scholars, the Theodor Wolff papers (now in the Bundesarchiv Koblenz), limited in scope though they are. The book's most interesting pages are on Wolff's meetings with Mussolini and on his relationship with Brüning. But, while adding a few details to our knowledge of Wolff's career, Sösemann's contribution is mainly one of interpretation rather than discovery. He presents a workmanlike examination of his journalists' attitudes to the deepening political and economic crisis, delineating agreements and differences, and concluding that his subjects did not really misinterpret National Socialism. Of course, they did make some mistakes, for example in their exaggerated support of Hindenburg in 1932; but, all in all, they retained their political integrity, intellectual honesty, and professional independence. Their exile after Hitler's accession to power was testimony to this. In the end, their fate demonstrated how limited, in fact, was their influence.

The book begs many questions, not least about the nature of a journalist's independence. The institutional context in which the four editors worked is sketched only in skeleton form, and, as a

result, Sösemann's assertions about his subjects' independence are not particularly convincing. If emigration can be used to support the thesis of independence, it can be used equally to underline that the last vestiges of independence had vanished. When reduced to its essentials, this book says that here were four by and large wise and astute voices crying in the wilderness. How they got there is not properly explained.

MODRIS EKSTEINS
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EDWARD L. HOMZE. *Arming the Luftwaffe: The Reich Air Ministry and the German Aircraft Industry, 1919–39*. Lincoln: University of Nebraska Press. 1976. Pp. xv, 296. \$14.95.

Edward L. Homze is the first historian to examine German aerial rearmament from 1919 to 1939 from the perspective of the aircraft industry. After briefly reviewing the benefits of the limited secret rearmament during the Weimar Republic for the later *Luftwaffe*, the study concentrates on Nazi aerial rearmament from 1933 to 1939.

Using largely neglected records of the Reich Air Ministry's Technical Office, Homze studies the government's development of the aircraft industry from one of modest dimensions to one of huge proportions controlled by the government. He traces this evolution within the contexts of the German economy, with its labor and raw material shortages, and of the Nazi regime, with its military theories, lack of coordination, and excessive play of individuals. He cogently argues that Germany's failure to develop a strategic air arm in the 1930s stemmed as much from the inability of the German aircraft industry and economy to support such an enterprise as from the Nazis' preference for tactical aviation. His incisive analysis of key figures in the Air Ministry, their roles and interaction, enables him to argue convincingly that Walther Wever's death and Hermann Göring's circumvention of Erhard Milch spelled the demise of rational development of the air force and industry by 1937. His overall assessment of Nazi management of the industry and, by extrapolation, the entire economy is negative. Their inability to guide the development of military aviation effectively meant that Hitler, who relied heavily on German aerial superiority in his decision to risk war in August 1939, embarked upon a policy of conquest with a flawed instrument.

If the work has a weakness, it lies in Homze's dependence for his discussion of the early 1920s on previous studies, which have slighted details of the industrial demobilization and contraction in favor of the clandestine rearmament. Had Homze seen

the personal papers of the postwar State Secretary of Air August Euler in the *Bundesarchiv*, his work on the early era would have been as informative as it is for the 1930s. Yet, in light of the book's focus on the Nazi era, this is certainly a minor flaw, as Homze's study of aerial rearmament and the industry from 1933 to 1939 is impressively researched, written, and argued. Indeed, it should stand as the definitive work in the field, one which clearly indicates why the *Luftwaffe* failed to measure up to its tasks in World War II.

JOHN H. MORROW, JR.
University of Tennessee,
Knoxville

HANS-ADOLF JACOBSEN. *Von der Strategie der Gewalt zur Politik der Friedenssicherung: Beiträge zur deutschen Geschichte im 20. Jahrhundert*. Düsseldorf: Droste Verlag. 1977. Pp. 372. DM 52.

The author and editor of some important books published in Germany on the National Socialist period has here collected sixteen previously published shorter pieces, many of which first appeared in periodicals difficult to locate. The footnotes have often been brought up to date. A few pieces, like numbers nine and eleven, are really too brief or too dated to warrant reprinting; only a few of the more important essays on German and European twentieth-century history can be commented on here.

The first piece, on continuity and discontinuity in German foreign policy, thoughtfully analyzes the issues on this currently much-debated question. The longest item, dealing with the structure of National Socialist foreign policy, summarizes many of the findings of Hans-Adolf Jacobsen's great book on the same subject. The sixth, dealing with the order to kill captured commissars and the mass murder of Soviet POW's, is a good introduction to that sad subject; there is now much additional information available in the German field command records. The piece on Stalingrad has some new material from the diary of General Wolfram von Richthofen. The one on the German collapse of 1945 documents a few of the more idiotic schemes considered in the face of defeat. Several pieces deal with the new army of the Federal Republic, while the last two concern German-Polish relations in the postwar era.

Most of the essays are distinguished by the fact that they raise questions for future research and are based on a comprehensive acquaintance with the German scholarly literature. There are still too many foolish assertions about the Treaty of Versailles, and readers will be astonished to find the Nuremberg trial verdict referred to as "summary"

when it is more than three times as long as the longest piece in this volume. More important, in the piece on World War II as a whole, which summarizes the book Jacobsen published in 1964 in the *Bibliothek für Zeitgeschichte* series, the author argues that the allies had a better strategic position for victory in 1943-45, when the essential fact is that the situations of the United States and the Soviet Union were *entirely* different: the basic strategic position of the U.S. with oceans on both sides was excellent for defense but very poor for offensive operations; that of the Soviet Union with land frontiers in the west and east was precisely the reverse.

The long piece on German public opinion and rearmament in the postwar years is exceedingly interesting. While brilliantly descriptive of the major currents and cross-currents, it is not very analytical. There is never any comment on the extraordinary attitude of those in West Germany who then and now identify themselves with every convicted war criminal—while simultaneously complaining about any non-Germans who make the same identification in collective judgments. There is also no discussion of the relationship of German rearmament to the original basic United States strategy for evacuating all or almost all of Western Europe in case of a Soviet attack, and the fact that only a massive West European contribution could lead to any serious effort to hold at the Rhine—to say nothing of defending any area east of the river.

Other readers will find other questions to raise, but they will be challenged by Jacobsen's suggestions and new perspectives. The profession will be grateful to have this convenient collection on German domestic and foreign policies.

GERHARD L. WEINBERG
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REINER POMMERIN. *Das Dritte Reich und Lateinamerika: Die deutsche Politik gegenüber Süd- und Mittelamerika, 1939-1942*. Düsseldorf: Droste Verlag. 1977. Pp. 377. DM 39.

This doctoral dissertation on Germany's relations with the Latin American states in the period 1939-42 is long on facts but short on interpretation. Reiner Pommerin's aims were to discover the Third Reich's intentions toward Latin America and how these intentions fitted into Germany's general plans for war; to point out the importance of economic relations and their impact on the German war economy; and to determine how much Hitler himself was involved and how Latin America fit into the plans for world domination.

The result, a diplomatic monograph of pre-World War II vintage, falls somewhat short of these aims. The book's usefulness lies in its compilation of the political and economic minutiae which primarily dominated German-Latin American relations during the early war years. Of equal value is the inclusion of a considerable amount of unpublished material from West German archives (the author was denied access to East German archives); the study would have benefited from the inclusion of similar material—on, for example, Latin American neutrality and Allied naval warfare—from United States, British, and Latin American depositories.

While Pommerin pays a great deal of attention to such relatively minor topics as attempts by German merchant vessels to break through the British blockade, the closing of German consulates, and the imprisonment of German nationals in various Latin American countries, his treatment of German fifth column and intelligence activities is superficial, and he does not add anything new to the well-known conflict between the Foreign Ministry and the *Auslandsorganisation*, the Nazi Party's organization for Germans living abroad. Most disappointingly, the author fails to make clear what impact, if any, trade with Latin America had on the German war economy. In the end, the author concludes that Germany's foreign policy toward Latin America was not based on any long-range plan, and that Hitler's interest in this continent was marginal at best. To spend 341 pages to arrive at this judgment seems a trifle excessive.

GEORGE O. KENT
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College Park*

LINA HEYDRICH. *Leben mit einem Kriegsverbrecher*. Commentary by WERNER MASER. Pfaffenhofen: Verlag W. Ludwig. 1976. Pp. 211. DM 26.

As one of Heinrich Himmler's chief lieutenants, Reinhard Heydrich was head of the NS Security Service (SD), the Gestapo, and the Criminal Police. He was, therefore, a chief executive in the machinery of police state terrorism and genocide. His widow, Lina Heydrich, has long been a widely used source for numerous histories about her husband, his organizations, and their work. The Ludwig Press has now made her memoirs available as a historical document, but to further inform their readers they have appended a corrective commentary by Werner Maser.

Unfortunately, as much of the book is devoted to the "hardships" of her life as to the undocumented side of Nazi history. She presents it all with a mixture of apparent naiveté, understatement, and

lapse of memory. Maser's primary concern was to protect the reader from any diminution of awareness of crime and inhumanity which might result from her version. Consequently, he did not perform the more valuable function of extracting and critically evaluating the potentially useful evidence. Instead, he veritably denies the validity of her version of Heydrich.

A complete dismissal is hardly justified. Like all memory sources, Frau Heydrich is subject to lapses and confusion. Some of these Maser properly corrects, but in overreacting to her rationalizations, he obscures what is potentially valuable. There is no complete denial of guilt; instead, the Heydrich that emerges is a complex person, the dutiful hangman often removed from planning and from much direct control over the forces shaping policy. As Shlomo Aronson has demonstrated (*AHR*, December 1972), the real Heydrich lay somewhat closer to this image than to that of a demonic mastermind of all evil between 1931 and 1945. Most significantly, Lina Heydrich has now committed her memory to paper so that all can evaluate it, and the uses to which it has been put by historians and journalists.

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MARC HILLEL and CLARISSA HENRY. *Of Pure Blood*. Translated by ERIC MOSSBACHER. New York: McGraw-Hill. 1976. Pp. 256. \$10.00.

Insofar as German National Socialism possessed a coherent ideology, racism stood at its core. Heinrich Himmler's SS constituted the instrument for the realization of Nazi racial policy in its most radical forms. The men of the "Black Corps" served both as the destroyers of racial undesirables and, with less success, as progenitors of a racially pure *Herrenvolk*. It is toward an elucidation of the latter aspect of SS racial responsibilities that Marc Hillel and Clarissa Henry ostensibly direct their efforts.

The dust jacket proclaims the book to be "The never before told story behind Hitler's secret program to breed 'The Master Race'." In fact, this work concerns itself with the SS agency known as *Lebensborn* ("Well of Life") with which Hitler had nothing to do, and whose "story" has been better told—and in far fewer pages—in an article by Larry V. Thompson.

Lebensborn provided assistance to model (and, therefore, large) SS families and operated maternity hospitals with services available to all mothers, married or not, who showed promise of producing racially "valuable" children. During

World War II, *Lebensborn* was involved with other Nazi agencies in the often forcible removal of children with desirable physical qualities from conquered territories for "Germanization." When dealing with a subject such as this there are many opportunities to sensationalize. Hillel and Henry have seized upon these opportunities with enthusiasm. Nowhere is this more obvious than in their tortuous effort to resuscitate the tired, but commercially exploitable, image of *Lebensborn* institutions as human "stud farms" for breeding the Nazi racial elite. SS sexual attitudes were, in fact, far more "bourgeois" than the authors would have us believe.

Hillel and Henry have attempted some serious archival research but have ignored the materials in the *Records of the Reich Leader of the SS and Chief of the German Police* and in other groups of SS documents which might have given their work coherence and value as an institutional study. Too often, the book is little more than a concatenation of lurid vignettes, each impossible to evaluate in the absence of footnotes.

This book seems to have been intended primarily to titillate, and to play upon the emotions of the general reader. It is of very limited value to the scholarly specialist.

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FRIEDRICH FORSTMEIER and HANS-ERICH VOLKMANN, editors. *Kriegswirtschaft und Rüstung, 1939-1945*. Düsseldorf: Droste Verlag. 1977. Pp. 420. DM 39.

The often neglected economic side of the Second World War served as the theme of a recent conference sponsored by the *Militärgeschichtlichen Forschungsamt* in Freiburg. Although the emphasis in this collection of papers from the conference is on Germany, the broader perspective reflected in its title and in its international array of contributors illuminates some intriguing questions which are less visible in a strictly national focus.

The explicitly comparative approach of two essays effectively highlights some of the ramifications of German policies. Willi A. Boelcke's theoretical and empirical discussion of war financing shows how silent methods of financing, such as artificial exchange rates and inflationary credit expansion, ultimately added to the hardships of the defeated populations. Alan S. Milward compares German and American efforts in the battle of productive capacity and suggests that the implications of Nazi racial philosophy regarding foreign workers and working women might account for the inferior German performance.

Even the essays with a narrower focus offer some important insights into economic factors in the war. Hans-Erich Volkmann's review of the drastic curtailment of German foreign trade during the first two years of the war shows that only the commercial expansion toward Southeastern Europe and the occupation of Western Europe rescued the German economy from critical shortages of raw materials. One of the shortcomings of the resulting *Grossraumwirtschaft* emerges in Horst Rohde's analysis of the German transportation network, which attributes most of the transportation bottlenecks to the fact that increasing demands on the railroads for war purposes continually outpaced increases in their capacity.

The contribution of other European nations to the Axis war effort also receives close scrutiny in this volume. According to Francesca Schinzing, the growth of German-Italian trade resulted mostly from the diplomatic isolation of the 1930s, since these two economies deficient in natural resources had little to offer one another. Klaus Wittmann defends Sweden's wartime trade with Germany as realism on the part of the Swedish government—it resisted German demands in small ways, but cooperated to the extent necessary to safeguard the nation's economic and political survival. Austria's importance as a bridge to Southeastern Europe and as a source of labor and raw materials predominated in German plans, which, as Norbert Schausberger demonstrates, only provided investment funds for projects essential to the war effort.

Three views of other wartime economies also extend the volume's comparative perspective. Bernd Martin summarizes Japan's difficulties in increasing production and concludes that Japan was an ally of little value to Germany. R. J. Overy describes the flexible and efficient administrative structure in Britain, and Detlef Junker discusses the politics of American economic controls. Intended for a German audience, these essays summarize research already published in English and familiar to specialists.

The final two essays on German occupation policies, however, are fraught with controversy. Harald Winkel points to several changing circumstances which influenced the French economy more than any deliberate German policy of exploitation. Waław Długoborski and Czesław Madajczyk offer a sophisticated interpretation of German exploitation of occupied territory in Poland and the Soviet Union which allows some subtle differentiation of interests within the German ruling classes, but still clings to the traditional framework of Marxist historiography. Although superficially agreeing that German occupation policies in Eastern and Western Eu-

rope differed considerably, these two essays represent antithetical perceptions of the inner workings of the Nazi regime.

By bringing together scholarship of varying perspectives, this collection constitutes a major advance in historical understanding. A further advance would be a similar fusing of sources. Długoborski and Madajczyk's reliance on materials inaccessible to Western scholars and their occasional misrepresentation of sources in Western archives underscores the problem. But if this volume does not provide all of the right answers, it does at last raise many of the right questions.

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WALTER VOGEL and CHRISTOPH WEISZ. *Akten zur Vorgeschichte der Bundesrepublik Deutschland, 1945-1949. Volume 1, September 1945-Dezember 1946.* Munich: R. Oldenbourg Verlag. 1976. Pp. 1,197. DM 168.

The Bundesarchiv of the Federal Republic of Germany in conjunction with the Institut für Zeitgeschichte has now begun publishing the documents pertaining to the "prehistory" of the Federal Republic. The reason for the long wait must have been simply to preserve the thirty-year rule. Bulky as it is and essential as it certainly will be for specialized research, the volume contains no revelations or general information of noteworthy consequence. During the period covered Germany was completely controlled by the occupying powers, hence the significant records passed into foreign archives. As the editors point out, what was left for the Germans, even those at the top level, was to talk about their economic misery and generate small glimmerings of political revival in the midst of all but total impotence.

With just a few exceptions, the forty-nine documents printed are the minutes of two organizations: the *Länderrat* (State Council) in the United States zone and the *Zonenbeirat* (Zonal Advisory Council) in the British zone. The *Länderrat*, composed entirely of appointed officials in the state governments, while it had no independent authority, was in the military government line of control and was sometimes called upon to legislate. The deputy military governor, Lieutenant General Lucius D. Clay frequently sat in at its meetings and one or more of his chief assistants always did. The *Zonenbeirat* was permitted two functions: to discuss and to advise (when the occupation authorities asked for advice). The higher British military government officers did not attend, and the German heads of state governments were members but did

not always attend. On the other hand, political parties, industry and trade, and unions were represented. Consequently, while the *Länderrat* minutes show the Germans working within the military government context, those of the *Zonenbeirat* tend to disclose more of what was going on outside the official channels.

Unfortunately, only a small part of the material is in the form of stenographic records, the rest being summaries, resolutions, and reports fleshed out to some extent by footnotes. There will, however, be at least three more volumes, and no research library can afford not to acquire the set.

EARL F. ZIEMKE
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ERNA LESKY. *The Vienna Medical School of the 19th Century.* Baltimore: Johns Hopkins University Press. 1977. Pp. xv, 604. \$18.50.

This monumental work ought to have been translated twelve years ago. Exact in detail, comprehensive in coverage, brilliant in posing comparisons, this book is a masterpiece of medical history. Although more than a thousand physicians are mentioned by name, the focus is seldom blurred: emphasis falls on the great researchers who founded and advanced medical specialties. Foremost among these is Carl von Rokitansky (1804-78), who created the science of pathological anatomy. Other giants include Rokitansky's colleague in internal medicine, Joseph Skoda, the anatomist Joseph Hyrtl, and the dermatologist Ferdinand von Hebra. These men and their students pioneered in founding medical specialties, a task which Erna Lesky sees as the crucial achievement of Viennese medicine.

Any history of medical thought and practice touches several audiences, including medical historians, physicians, and general historians. Medical historians will find the book a model of precise method; the frequent comparisons with French medicine are especially illuminating. Physicians will be interested mainly in their own specialties, which are arranged conveniently by chapter. General historians may wish, however, that the framework of analysis were somewhat broader. Adepts of Michel Foucault may cavil that he exerted no influence on Lesky. More important perhaps, one learns too little about Billroth's musical activities, about Feuchtersleben's literary accomplishments, or about Ernst Brücke's writings on art. The fact that these matters are mentioned but not explored is the one disappointment in this excellent volume.

What of the translation? We have here a workmanlike, if sometimes wordy, version of the unrevised 1965 text. The already ample bibliographies

(albeit not the text) have been updated through 1974, and all German book titles appear in both German and English. With such pains taken to oblige English-speaking readers, which parts of the book, it may be asked, are most likely to impress us? Americans will surely prize the chapter on post-1850 psychiatry, in which Freud surfaces amid a welter of forgotten predecessors and colleagues. The section on Billroth (pp. 392–405) breathes a compassion which reminds us what humanism in medicine can mean, and the section on Rokitansky (pp. 106–16) evokes the heroic age of medical science in a way that dwarfs many later achievements. Indeed, Lesky shows a special affinity for Rokitansky. While it would be presumptuous to call her or anyone else the Rokitansky of medical history, she comes close to rivaling him in zeal, indefatigability, and vision. Of few scholars in any field can as much be said.

WILLIAM M. JOHNSTON
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G. R. POTTER. *Zwingli*. New York: Cambridge University Press. 1977. Pp. xvii, 432. \$39.50.

Those mindful of Zwingli's influence on English theology and churchmanship, especially during the reign of Edward VI, will experience a sense of satisfaction that an English historian has in part repaid the debt with a biography that will stand for many years as the best portrayal and analysis of the "third man" of the Reformation. The dominant picture of Zwingli is that of the Zurich church historian Walther Koehler, who saw him as a humanist (he seemed to wish to find a liberal among the magisterial reformers) and a reformer much in debt to Luther, the two elements in interior conflict. Oskar Farnet, for decades pastor in Zwingli's minster, on the other hand, depicted him too much in local terms and missed his broad influence and general significance.

George R. Potter, who invested many years in the study of Zwingli's works, the vast literature, and the local archives, enjoyed the advantage of distance and has put it all together in a truly masterful biography. He sees Zwingli essentially as the Swiss prophet, a systematic and tough-minded man who moved to put his reformed ideals into effect in an astonishingly brief time—at age thirty-two he was still a common priest and at age forty-six he lay dead on the battlefield of Kappel. The author properly assesses the importance for Zwingli of Erasmian humanism, of the city-state and Swiss confederacy setting, of the Lutheran Reformation, and above all, of his intense preoccupation with the Scriptures. There emerges

from this volume a Zwingli who was not the powerful figure Luther was, nor the brilliant theologian Calvin was, but a determined, steady, city-state reformer, who was shocked to encounter the Anabaptist radicals spawned within his movement. On the much controverted question of Zwingli's views of church and state, the author proves his mastery, for he depicts the theocracy which Zwingli advocated, not as one of church over state or Zwingli over Zurich, but rather the rule of God over both church and state.

This is not hagiography, for the author tells us that Zwingli lacked the courage to face Eck and a hostile Catholic audience in 1526, and that he drifted into political machinations and notions of preventive war, which led to the catastrophe of Kappel. Nor is the book flawless, any more than Zwingli himself. Carlstadt, for example, did not reach Zurich on his first exile from Wittenberg, but only later, a point not made clear and one which distorts his possible influence on Sacramentarianism. Nor does the word "consubstantiation" adequately describe Luther's sacramental doctrine. He did not believe that baptism removed sin, but only guilt, and he had quite as high regard for reason as did Zwingli. But Zwingli himself, a very high predestinarian, was more concerned with his entry in the Book of Life than with what even a very great English historian might say of him.

LEWIS W. SPITZ
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LEO SCHELBERT. *Einführung in die schweizerische Auswanderungsgeschichte der Neuzeit*. (Schweizerische Zeitschrift für Geschichte, number 16.) Zurich: Verlag Stäubli. 1976. Pp. 443.

Emigration has played an important role in Swiss history, particularly in preindustrial times, when population pressure on scanty resources led to a persistent and sizeable outflow from the small country. Between 1400 and 1800 more than a million persons—just about one half the entire natural increase (excess of births over deaths)—was drained out of the country. By far the bulk of this early outward movement took the form of mercenary service; the Swiss emigrated not as civilian settlers or colonists but as mercenary soldiers selling their professional services impartially to all European powers who could use them.

Although the origins of civilian emigration can also be traced as far back as the Middle Ages, it remained entirely secondary until the nineteenth century when mass movements of civilians finally replaced the mercenary system. Prior to the nineteenth century many of the civilian migrants were

religious refugees—members of various sects, Anabaptists, Mennonites, Pietists, and others escaping cruel persecution by the conservative state churches of certain Protestant cantons, such as Zurich and Berne. Later on the predominant causes were economic, agricultural, and industrial depressions that led nearly half a million Swiss to leave their native land during the last century and a half.

This colorful history has given rise to a voluminous literature, but most of the historical accounts are specialized monographs dealing with limited aspects of the phenomenon. Condensed overviews of emigration also exist in general Swiss histories and in the demographic literature, but there has been no comprehensive treatment of Swiss emigration per se. Leo Schelbert, a Swiss scholar teaching history at the University of Illinois in Chicago, has made an attempt to fill this gap. Calling his book *An Introduction to Swiss Emigration History in Modern Times*, the author has drawn extensively upon archival sources and other documentary materials in addition to surveying a wide range of secondary literature.

The book consists of four parts. In part one Schelbert discusses migration theories drawn from the demographic literature and also considers the problems of causation and motivation from the point of view of the individual migrant. He next portrays migration as a process, i.e., the various steps a migrant had to take in leaving home, the means of transportation available in different periods, the role of emigration agencies, the hardships of the journeys, and the conditions facing the migrant in various countries of destination. Throughout, the perspective is that of the individual migrant, illustrated by documentary evidence.

Part two presents a historical and statistical overview of both military and civilian emigration, separated by areas of destination. Part three offers a selection of official and personal documents designed to afford the reader a vivid sense of the experiences of Swiss emigrants in a variety of circumstances. These documents consist of letters and reports from Swiss soldiers and settlers in different parts of the world and at different times during the eighteenth and nineteenth centuries. Also reprinted are some official documents illustrating the attitudes and actions of government authorities in Switzerland and in several receiving countries. The last part of the book contains a discussion of the type and quality of source materials to be found in various archives and an extensive annotated bibliography of the secondary literature.

One may quibble with some of Schelbert's interpretations. Focusing on the individual migrant, he tends to understate the role played by economic

and demographic background factors. But these are matters of emphasis which do not seriously detract from the merits of the book. The skillful use of documentary sources contributes to a better understanding of the migration experience, not only in the national context of Switzerland but in Europe as a whole.

KURT B. MAYER
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GENE BRUCKER. *The Civic World of Early Renaissance Florence*. Princeton: Princeton University Press. 1977. Pp. xii, 526. \$25.00.

This book makes two major contributions to Renaissance studies. First, it provides a detailed description and interpretation of the development of the Florentine political system from 1378 to 1430, based on the author's unrivalled mastery of the sources. Second, it sets forth in its central chapter—"The Florentine *Reggimento* in 1411"—an explanatory model of the social and economic relationships upon which, in the author's judgment, the political system rested.

Conceptually, the volume is a continuation of Brucker's *Florentine Politics and Society, 1373-1378* (1962). Here again, the records of the *pratiche*, political deliberations of leading Florentine citizens, are systematically mined for evidence of changing attitudes toward internal political and fiscal problems, foreign affairs, and the nature of governance itself, as well as fairly detailed prosopographical material on the distribution of power and influence in the Republic. But Brucker's task is far more complex here than it was in his earlier work. The period from the revolt of the Ciompi to the advent of Cosimo de' Medici, apart from being one of the principal battlefields of Florentine historiography, involves far subtler relationships between political, fiscal, administrative, diplomatic, and intellectual developments. No one is more aware of this than the author. His claims are modestly formulated and generally supported by impressive amounts of evidence. His differences with other historians are noted, but not stressed; not the least of the book's virtues is that it is entirely free of the jejune polemics with which the nest of Florentine history has been fouled in recent years.

The process Brucker traces is the transformation of the Florentine regime from a polity dominated by corporate institutions (guilds, confraternities, papal and antipapal factions) to one in which a "leadership elite," functioning primarily as individuals, helped to steer the city through the recurrent crises which "each year brought" (p. 283). This elite is identified, and its evolving new style of political behavior followed, through the *pratiche*. In these debates, of which we know the content from

summaries made by notaries, Brucker discerns the workings of an especially powerful "inner circle," normally numbering well under one hundred men. He characterizes them as "experienced statesmen, recruited largely but not exclusively from the great families" (p. 501). Owing to their need to take into account the views and interests of the larger community, Brucker maintains, this group did not constitute an oligarchy. Some may fail to be entirely convinced by the author's efforts to show that the "leadership elite" which he so elegantly and thoroughly documents was not in fact a ruling class. (Here the lack of a comparative dimension is unfortunate, particularly since the author himself recognizes that much previous Florentine history is "characteristically parochial and non-comparative" [p. 4]). Even dynastic rulers in the Renaissance had popular constituencies they could ill afford to alienate, but no one would argue from this that such rulers were not monarchs. Indeed, the "elitist regime" Brucker describes seems compatible with a standard dictionary definition of oligarchy, as "a form of government in which the power is vested in a few persons or in a dominant class or clique; government by the few."

Admittedly the Florentine elite could have been even smaller, more narrowly based, more self-serving or short-sighted, and far less committed to both the rhetoric and the substance of civic and republican ideals. But Brucker's own evidence, and the earlier contributions of Martines, Herde, and Becker, tend to support the view that the most powerful figures in Florentine politics, society, and civic culture during these years had a very real interest in not appearing to be as powerful as perhaps they were. There are serious methodological problems in how to use and assess their public utterances, and here even Brucker's almost always careful and restrained inferences may be debatable.

Brucker is at his best when he is working close to the sources, and that is most of the time. He evokes the texture of *pratica* debates with enormous skill, deftly setting them in their complicated and rapidly shifting contexts. Individual Florentine leaders begin to emerge with clearly identifiable personalities and styles. His splendidly crafted narrative will be the standard political history of Florence from 1378 to 1430 for years to come, and his interpretations should stimulate the kind of informed debate that only a magisterial synthesis can produce.

WERNER L. GUNDERSHEIMER
University of Pennsylvania

RONALD M. STEINBERG. *Fra Girolamo Savonarola, Florentine Art, and Renaissance Historiography*. Athens: Ohio University Press. 1977. Pp. 151. \$11.00.

A story told in the Florentine archives concerns the arrival of a young American graduate student to investigate the problem of whether Chaucer had visited Florence. After a year the American departed without finding a jot of evidence. Florentine archivists who enjoy happy endings assured me that the young man received his doctorate; the thesis listed all the sources he had consulted. Positivistically minded historians could indeed defend the bestowal of a degree on grounds that the dissertation made a contribution to scholarship, albeit somewhat negative. After all, subsequent researchers would not have to read the same sources, but could go on and systematically gather new non-evidence.

The present monograph belongs to a grand tradition wherein authors seek to convince readers that the emperor has no clothes and, moreover, must remain nameless. Ronald M. Steinberg sets out to prove that Savonarola had no "general and pervasive influence on Florentine art." He attacks claims of earlier scholars who, in his opinion, have made these sweeping assertions. Toppling his scholarly forebears, the author would make historical bricks from straw men. But such construction is no longer possible. With the publication of Donald Weinstein's elegant and nuanced *Savonarola and Florence* (1970), it became clear that Dominican and city had interacted in tangled ways. The preacher was responsive to Florentine civic interests and patriotic aspirations but cast them against an eschatological backdrop. On the one hand he reassured his audience of the legitimacy of their wealth, while on the other he severely chastised them for acquisitiveness. In all this it is difficult to sort out what was traditional and Florentine from what was Dominican and Savonarolan. Prophecy, republican millenarianism, a vigorous Joachite tradition—these were but part of the web binding preacher and citizen together.

Ordinarily one would not wish to challenge an author so strenuously, particularly when contemporary testimony was so ambiguous. But what is at issue here is an argument that seeks to deny influence by reducing a prophetic and millenarian movement of inordinate complexity to a single strand, only to cut that strand so that the art is allowed to float free as a balloon in some timeless, ahistorical space. If, according to the author, we cannot find a direct and exact match between Botticelli's iconography and the imagery of Savonarola's sermons, then no connection exists. The deeper question of why both men were responding with such apocalyptic fervor to happenings in their own times has substantial bearing on the history of culture in general and art in particular. Prophecies and their imminent fulfillment were evident in sermon and painting; so too were the themes of mil-

lenarianism and the divine election of Florence. But the spirituality of artists has received scant attention. In recent years the lives of artists have been viewed under the rubrics of science, techniques, and cool problem-solving. The contemporary biographers of Michelangelo and Botticelli would not have viewed them thus; nor would the artists have understood themselves in this modernist perspective. To separate art from its religious context or artists from their spiritual preoccupations on grounds that direct influences cannot be proven can only give radical empiricism a bad name.

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PETER PARTNER. *Renaissance Rome, 1500–1559: A Portrait of a Society*. Berkeley and Los Angeles: University of California Press. 1977. Pp. 241. \$14.95.

Whoever is familiar with the admirable blend of scholarship and elegance characteristic of Peter Partner's previous studies of the Eternal City may be somewhat surprised by this one. The exposition is occasionally marred by unnecessary repetitions (the condition of the Jews is described twice, the entry of Charles V in 1536 three times), and it frequently suffers from the substitution of imprecise adjectives for concrete descriptions, such as the "monstrous" accumulation of clerical incomes, the "fine" fountains of the Villa d'Este, and the "horrible" sufferings of the besieged Florentines. The sources—judged, in the absence of footnotes, from the text alone—seem to be restricted to the few monographs listed in the two-page bibliography and to a random selection of printed documents: two or three contemporary travelogues and guidebooks, a volume of occasional verse, and the letters of Pietro Aretino. There are no references to the abundant archival collections pertinent to the period—papal, communal, or private—and there are very few references to the many specialized articles published in the relevant journals.

This restriction in sources may well be responsible for several notable lacunae. Most of what Partner says about administrative and economic affairs comes from Jean Delumeau, who admittedly concentrates on the second rather than on the first half of the century. He does not make use of the kind of material presented, for example, in the recent study of the governor's office by Niccolò Del Re. What he says about religious life is largely dependent on Philip McNair and Romeo de Maio, who deny the existence of an Italian "Catholic Reformation." He says nothing either about the Oratory of Divine Love, which was fully documented by Pietro Tacchi Venturi, or about the

most popular religious leader of the age, Filippo Neri. Partner ignores the work of the archeologists and classical scholars who became the most active members of the Roman intellectual world in the following generations. And he accepts uncritically many of the now questionable commonplaces about the repressive character of the Counter Reformation, such as the "despotic and terrorizing" Inquisition (p. 45) and the demoralizing effects of a "loss of Italian liberty."

This restriction in sources may also account for a number of minor factual errors: the election of Leo X did not precede the fall of the Soderini regime; Filippo Strozzi was not "tortured in the prisons of Cosimo I" (p. 86); Paolo Giovio was never a "papal historiographer," particularly not in 1552 (p. 26); the Council of Trent did not assemble in 1542, etc. And it may account for some rather questionable judgments: that Cosimo I was "hardly more than an agent of Charles V" (p. 28), *pace* Giorgio Spini; that Egidio da Viterbo was a "windbag" (p. 16), *pace* John O'Malley; that Siena was "ground cruelly into the dust," *pace* Roberto Cantagalli; that Paul IV's character was determined by the mountain origins of his family (p. 42).

Unlike its predecessors, however, and unlike its recent companion for the thirteenth century by Robert Brentano, this book is obviously not intended to be a definitive work, or even a survey of current scholarship, but rather a preparatory essay or outline for further research. As such, it has considerable merit. For one thing, it clearly labels as anachronistic a number of commonly accepted judgments about the prevalence of "paganism" at the time, about the incompatibility of humanism and Christianity, and about the regressive character of "mannerist" art. For another thing, it proposes some very promising methodological innovations. A "portrait of a society," Partner insists, must include all its elements—tanners, butchers, and innkeepers as well as cardinals—and it must not be broken down according to modern academic disciplines, in such a way, that is, as to separate the art-historical study of buildings from the sociological study of the people who inhabit them. Best of all, this book puts forth several very provocative working hypotheses. The papacy after Alexander VI, Partner suggests, was a very different institution from the papacy before the Great Schism. Its chief achievement lay in its adoption of humanism—literary and philological as well as architectural—as its official ideological support. And it successfully used humanism to reestablish Rome as the center of Catholic Christianity at a time when theology (the instrument it had used effectively toward the same end in the eleventh century) had lost its persuasive force.

Partner still feels a certain antipathy toward the new age he has chosen—an age when popes of “rather inferior moral stature” tolerated the “illicit,” “immoral” behavior of prostitutes (p. 153), when “Renaissance despots . . . imposed . . . great decorative schemes on their supine peoples” (p. 173), and when “self-indulgent men” like Giovanni Della Casa (p. 62) could rise to the episcopate. But the program proposed in this book cannot but awaken in the reader a sincere hope that he will soon overcome these antipathies—and that he will go on to do for the Rome of Clement VII and Paul III what he has already done so brilliantly for the Rome of Gregory VII and of Martin V.

ERIC COCHRANE
University of Chicago

MANFRED EDWIN WELTI. *Giovanni Bernardino Bonifacio, Marchese D'Oria, im Exil, 1557–1597: Eine Biographie und ein Beitrag zur Geschichte des Philippismus.* (Travaux d'Humanisme et Renaissance, number 150.) Geneva: Librairie Droz. 1976. Pp. 317.

Since Giovanni Bernardino Bonifacio, Marquis of Oria in the Neapolitan Kingdom, was one of the few sixteenth-century Italian followers of Luther about whom something is known, he has a claim to the attention of specialists in Italian religious history. His long life can conveniently be divided into two equal halves. He spent the first, from 1517 to 1557, in his homeland, and the second as a restless exile in Northern Europe. Having used his substantial wealth on travel and books, he died poor and blind in Danzig in 1597.

The present work represents the concluding volume of a projected biography of Bonifacio; the first volume has yet to be written. This procedure leaves the reader in something of a quandary, since Bonifacio's earlier life, leading to his conversion to Protestantism, is not discussed. For a good brief treatment of this period the work of Frederic C. Church, *The Italian Reformers, 1534–1564* (1932; reprint, 1974) is still useful.

Welti has done a prodigious amount of careful research in order to document the itineraries of his peripatetic subject and to shed light on Bonifacio's acquaintances and correspondents (mostly Italian exiles, with the notable exception of Bonifatius and Basilius Amerbach in Basel). One can imagine how strange Bonifacio must have seemed to contemporaries as he repeatedly traveled between Switzerland and Poland with two North African female servants (most likely slaves), assorted dogs, and a library which had reached 1,100 volumes by 1574, all without speaking any Germanic or Slavic language! Written in a concise, at times epigrammatic style, the book abounds in exact information not only about Bonifacio's journeys, but such di-

verse matters as the cost of transporting books, his dietary preferences, his sizeable investments (on which, for reasons of conscience, he refused to take interest after 1574/75), or his problems with his exotic servants. Bonifacio emerges as a restless, difficult man, suffering from depressions and fear of persecution, and incapable of forming close human relationships.

Strangely enough the author gives little information about the subject which would be of greatest interest to potential readers, the nature of Bonifacio's thought and religious convictions. Perhaps this central topic will be treated in the first volume, or illuminated by Welti's forthcoming edition of Bonifacio's letters. In the present book there is but a brief section in an appendix, reprinted from a journal article, on Bonifacio's theological views. They are surmised from marginalia and markings in some of his remaining books.

As suggested in the subtitle, the book also has the aim of making a contribution to the history of Melanchthonian Lutheranism. But the contribution is relatively minor. Bonifacio did not play a significant role among Melanchthon's spiritual heirs. He is considered one of them on the strength of his humanistic education, his adherence to the Augsburg Confession, and the general nature of his sympathies.

In sum, this is a book about a man who, for all of his interesting life, was still intellectually mediocre, as the author acknowledges (p. 253). It demonstrates Welti's superb research techniques and imaginative detective-work (for example, in the attempt to decipher the meaning of some letters appearing on a medal of Bonifacio, pp. 105–10), and contains a great deal of valuable material, but rightly does not claim to deal with more than a case which was “curious, but not major in Reformation history” (p. 243).

ELISABETH G. GLEASON
University of San Francisco

ANTONIO CONFALONIERI. *Banca e industria in Italia, 1894–1906.* Volume 3, *L'Esperienza della Banca Commerciale Italiana.* (Studi e Ricerche di Storia Economica Italiana nell'Età del Risorgimento.) Milan: Banca Commerciale Italiana. 1976. Pp. 601.

This third volume of Antonio Confalonieri's study focuses upon the most important of the “mixed banks” operating on the German model and providing industrial credit in Italy, the Banca Commerciale Italiana. It examines the origins and development of this pace-setting institution as well as its interrelationship with a number of sectors of the industrial economy. Commencing with an examination of the influx of German capital by means of the founding consortium, the author then traces

the financial and territorial growth of the institution. Particular attention is given to Otto Joel, first director of the BCI, who played a crucial role in bringing in foreign capital as well as molding the nature of the bank.

Confalonieri's presentation of the conflict between Joel and Edouard Noetzelin of the Banque de Paris et des Pays-Bas, then President of the Central Committee of the BCI, is revealing. It emphasizes the different approach of the German model banks from those set up with French capital. The Joel/Noetzelin polemic also pinpoints the division that existed between the general administration and the management of the BCI on the nature and degree of participation in industrial enterprises, with the Bank's foreign associates tending to insist on liquidity and questioning the long-range industrial operations proposed by Joel. Ultimately this was symptomatic of a broader conflict between entrepreneurial autonomy favored by Joel and institutional control required by Noetzelin.

The Bank followed a *via media* between the demands of the international credit system and the needs of the national economy. The BCI did make credit available to a number of firms in addition to providing brokerage services for newly formed joint stock companies or new issue securities for established corporations. But the extent of the Bank's direct intervention in the construction of new companies or the transformation of others remained modest (usually five to ten percent of the capital involved). Still, the BCI played a part in the entrepreneurial activity of the firms financed and sought unqualified control over the financial dealings of such companies. Confalonieri's description of the precise relationship of the Bank to a series of industries is most valuable.

This volume shares many of the attributes of the first two. Here, as before, the political factor is not strongly emphasized, at times virtually ignored. This volume, however, like the others, is rich in detail and documentation, drawing upon the archives of the BCI, until now largely unexplored, as well as the papers of many other institutions. The statistical appendix contains twenty-seven tables with an additional sixty-two interspersed within the text, providing information previously unavailable. The documentation, supported by a lucid analysis, serves to render this the most important assessment to date of the Bank's role in guiding Italian corporate development from 1895 to 1906.

FRANK J. COPPA
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JOHN WHITTAM. *The Politics of the Italian Army, 1861-1918*. Hamden, Conn.: Archon Books. 1977. Pp. 216. \$12.50.

Since there are no studies in English on the Italian army, this is a useful survey because it gathers scattered bits of information into one volume. John Whittam's thesis is that the army played an important, sometimes preponderant, role in the history of Italy between 1861 and 1918. In order to support this contention, he stresses the use of the army to tame the south and Sicily in the 1860s and in the 1890s. He also concentrates on the reforms of Generals LaMarmora and Fanti, both of whom collaborated with conservative politicians to oppose Garibaldi's concept of the nation-in-arms and to create a trustworthy professional army. In addition, the author recounts how General Ricotti Magnani reorganized the army in the 1870s in order to use its structure as a weapon against regionalism and to build a force capable of defending an independent Italy.

Whittam bases his work almost exclusively on secondary sources. The absence of primary sources may explain why the book contains almost no analysis. He mentions, for example, the close relationship among the leaders of the government, the military, and industry, but he never analyzes how this elite worked to provide lucrative government contracts that promoted infant industries, increased the size of the military establishment, and lined the pockets of the politicians. Nor does he examine how the sizable amount of the state budget spent on the military infuriated the anti-militarists, who viewed the army as the tool of capitalist oppressors. The socialists, in particular, constantly argued that the government should spend less on the military and more to alleviate the ignorance and misery of the masses. An analysis of these factors, which certainly exacerbated tensions between the army and the lower classes, is imperative for an understanding of Italian history between 1861 and 1918.

Since the book contains too little analysis to interest the specialist, one assumes that Whittam aims at a more general audience. Persons searching for information, however, will have difficulties, since the author often presumes too much knowledge. He mentions, for example, the Socialist party's policy of *né aderire, né sabotare* (which he incorrectly calls *non aderire, né sabotare*, p. 195), but he gives no indication of what that policy was or why the Socialists promoted it. The author also evidently believes that everyone reads Italian well enough to understand the numerous statements left in the original. It is doubtful that this is true in Great Britain; certainly it is not true in North America.

CHARLES L. BERTRAND
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ÉMILE POULAT. *Catholicisme, démocratie et socialisme: Le mouvement catholique et Mgr Benigni de la naissance du socialisme à la victoire du fascisme.* (Religion et Sociétés.) Paris: Casterman. 1977. Pp. 562.

This is a meticulously researched biographical study by a French sociologist of a fascinating though little-known Italian priest, Monsignor Umberto Benigni, who served as kind of *éminence grise* behind Pope Pius X (1903–14) during the latter's campaign against the modernist current within the Roman Catholic Church. The product of fifteen years of archival study, Émile Poulat's tome is a companion-piece to two earlier documentary works he has brought out on Benigni: *Intégrisme et catholicisme intégral: Un réseau secret international anti-moderniste, La "Sapinière" (1909–1921)* (1969) and a reprint edition of Benigni's journal, *La Correspondance de Rome* (1971). Like its forerunners, this book provides a great deal of information about the reactionary *intégriste* current within Catholicism. Until Poulat began his investigation, the activities of Benigni were largely shrouded in mystery. Few Italian writers had given him more than passing mention.

In examining Benigni's career from his birth in Perugia in 1862 to his death in Rome in 1934, Poulat is chiefly concerned with explaining the Italian priest's intellectual development within the broader ideological context and shifting sands of the social Catholic movement "from the birth of socialism to the victory of fascism." Though wordy at times, the book is a valuable study, admirable for its careful analysis, numerous explanatory notes, appendixes, and cross-indexes.

In the first part of his study, Poulat comments briefly on Benigni's early years as a priest in Perugia where, under the inspiration of Leo XIII (his fellow townsman), he began to manifest an interest in social Catholicism. Benigni soon shifted ideologically to the right, however, becoming an uncompromising *intégriste*. He also embarked upon a journalistic career that was to lead him to Rome in 1895. There he launched a series of ultraconservative journals while also teaching Church history in various seminaries and academies.

After Pius X's ascension to the papal throne, Benigni's professional career advanced rapidly. In 1904 he entered the Roman Curia as minute-taker of the Congregation of the Holy Faith, a position he held till 1911. In January 1904 one of Benigni's journals popularized the term "modernism" to describe the hated antifundamentalist current associated with Alfred Loisy and others. Three years later Pius denounced the "pernicious" doctrines of modernism in the encyclical *Pascendi*. By this time Benigni had become a close aide to Cardinal

Merry del Val, the secretary of state, and was for a time also the pope's "domestic prelate." Meanwhile, Benigni's journal, *Corrispondenza romana* (1906–09), subsequently relabeled *Correspondance de Rome* (1909–12), was busily engaged in denouncing the dangers of innovation within the Church. With the Curia's knowledge, Benigni founded in 1909 the secret international league, "*Sodalitium Pianum*"—more familiarly known as S.P. or la "*Sapinière*"—for the purpose of ferreting out and discrediting liberals, democrats, and modernists within the Church. Benigni named this fellowship in honor of the Counter-Reformation pontiff Pius V (who, incidentally, was the last pope to be canonized prior to Pius X). The "*Sapinière*" worked closely with *Action Française* in France and had agents in Italy, Belgium, Germany, and Eastern Europe.

Opposition to Benigni developed by 1911 in certain quarters. Sharp complaints from Church leaders in Bavaria caused the Vatican to terminate the *Correspondance de Rome* late in 1912. The death of Pius X in 1914 and the advent of his more moderate successor Benedict XV also worked against Benigni. But he remained intractable, even after 1915 when the Germans, during their occupation of Belgium, seized the codebook of one of the "*Sapinière*" agents and spread considerable unfavorable publicity about the conspiratorial organization. In the spring of 1921 renewed adverse publicity in Paris forced the Vatican to undertake an investigation. Benedict eventually suppressed the "*Sapinière*" on November 25 of that year. Nonetheless, Benigni persisted in his *intégriste* campaign and was still lending support to *Action Française* through a number of press agencies at a time when the Vatican was at last turning its back on that monarchist organization.

After Mussolini's rise to power in Italy in 1922, Benigni became an enthusiastic supporter of the Fascist regime. On occasion, it seems, he even supplied its police with information. Poulat characterizes Benigni, however, as a Fascist Catholic rather than as a Catholic Fascist. The priest's last years were spent working on a multivolume *Storia sociale della Chiesa* that got as far as the fourteenth century. Benigni's death on February 24, 1934 passed unmentioned by either *L'Osservatore Romano* or *Civiltà cattolica*, for in their eyes, as Poulat explains, he "no longer had existed for some time."

CHARLES F. DELZELL
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JOHN N. MOLONY. *The Emergence of Political Catholicism in Italy: Partito Popolare, 1919–1926.* Totowa, N.J.: Rowman and Littlefield. 1977. Pp. 225. \$15.00.

John N. Molony, of the Australian National University, has written the first monographic history of the *Partito Popolare Italiano* to be published in the English language. As such, it will be a useful addition to the more general English-language work of Richard Webster and the more detailed studies by Gabriele De Rosa and Edith Howard which, unfortunately, have never been translated from the Italian. Molony's study is more thoroughly researched than is Webster's and he is more disinterested and impartial than De Rosa, the acknowledged doyen of PPI scholarship, whose works read like a brief in the defense of Luigi Sturzo, the PPI's founder.

Like De Rosa and Howard, Molony's purpose is to recount the sorry tale of the PPI's meteoric rise and fall in the turmoil and violence of the postwar years which saw a desperate, weary Italian electorate gradually turn to the Fascists almost by default as the only visible alternative to civil war and anarchy. Molony agrees with the prevailing view that Mussolini's rise was utterly resistible and that the PPI must share the blame (with many others) for making the resistance so ineffectual. The demise of the PPI itself was partly a case of suicide, for the party could never resolve the conflict between its avowed aconfessionality and the dependence of the party upon the parish clergy and the devout laity, both of whom were obliged to obey ecclesiastical authority in matters touching upon faith and morals. Thus the Vatican's support, or at least its benevolent neutrality, was essential. Unfortunately, the Vatican was becoming disillusioned with the PPI even before the accession of the ultraconservative Pius XI in 1922. After that date the Vatican became more and more openly hostile to Sturzo's party and increasingly benevolent toward Mussolini's. In the end, "the Italian fascist state and the Vatican worked hand in hand to help destroy the Partito Popolare" (p. 12). The Liberals and the Socialists hated the PPI almost as much as the Fascists did and saw too late how important this party was to any democratic government strong enough to stop the Fascist avalanche.

These harsh but evenhanded judgments are delivered in a lucid and readable prose which occasionally recaptures the drama of these sordid events. Objections can be raised to some of these judgments as well as to other aspects of this book. I find the interpretation of the Giolittian era to be simplistic and grossly one-sided in its condemnation of Giolitti's policies. One might also wish for more than brief, tantalizing glimpses of the day-to-day operations of the PPI on the local level. The complete absence of any systematic analysis of the social structure or the geographical distribution of the PPI electorate will strike many

as a serious deficiency. On the other hand, Molony's work is well researched, interesting, and notably fair-minded, with no visible ideological ax to grind.

RICHARD CAMP
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BARTOLO GARIGLIO. *Cattolici democratici e clerico-fascisti: Il mondo cattolico torinese alla prova del fascismo (1922-1927)*. (Saggi, number 167.) Bologna: Il Mulino. 1976. Pp. 294. L. 6,000.

Bartolo Gariglio provides a thorough description of the way in which Turin's Catholic organizations confronted Fascism from 1922 to 1927. Four separate chapters recount the experiences of the Popular Party, Catholic labor organizations, the Catholic press, and Catholic Action. Sources for all four accounts are extensive and include a wide range of contemporary journalism and ecclesiastical as well as state archives.

The result is a clear delineation of the currents within Catholic Turin and of their relative strengths. We see not only connections between ecclesiastical, economic, and political organizations, but also the effects of such events as the failure of their rural banks on the resistance of Catholic peasants to "fascistizzazione." We learn that although democratic elements in these organizations were stronger than the clerico-fascists, the hierarchy had the controlling hand and by 1926 moved, however reluctantly, to replace democrats with clerico-fascists or men acceptable to the fascists.

Gariglio's chapter on the final struggles of the local PPI is the most engrossing; he is scrupulously exact in showing the divisions, frustrations, and limited initiatives of the democratic majority in a situation heavily weighted against it. Fascist violence, now directed against the moderate parties as the last effective rivals, only drove the PPI further to the left and at the end of 1924 there was still a coherent antifascist alliance. But by 1925 resistance was largely limited to words, and since appeals for constitutional liberties went unheeded and even an assembly in favor of freedom of the press was prohibited, this alternative was limited indeed.

Informative and revealing as it is, this study is disturbingly incomplete. Perhaps it is because the choice of dates deprives the account of a beginning and an end; or perhaps casting the action into four chronological narratives, which repeat the same years each time, leaves it without a continuous and unified story line. Even more critical is Gariglio's reluctance to pose the larger questions that would

give these local events their national significance. Was Salvemini right, for instance, in arguing that Catholics could not be consistent defenders of democracy in the Italian situation? It was in these years that, among other things, Mussolini effected a transition from negotiations between independent political parties to negotiations between himself and relatively autonomous institutions such as the Church and the army. This was surely the case at Turin where Cardinal Archbishop Gamba emerges as the protagonist of the Catholic forces by 1926, but neither the significance nor the context of this development is clear in Gariglio's account. For the informed historian of fascist Italy, Gariglio has drawn a picture with Dürer-like clarity that provides much detail about a particularly tangled underbrush of local politics. For the general reader the picture lacks the significance—and even the intrinsic tragedy of its subject—that might have been provided by a more encompassing thesis.

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RICHARD CLOGG, editor and translator. *The Movement for Greek Independence, 1770–1821: A Collection of Documents*. (Studies in Russian and East European History.) New York: Barnes and Noble. 1976. Pp. xxiii, 232. \$23.75.

In an age long accustomed to teaching and studying the past through short published selections of its written traces, this is the first such volume on modern Greek history to appear in English. In taking this long-needed step, Richard Clogg, an eminent historian of modern Greece from the University of London, chose a seminal period and an important subject. As he states in his preface, “the Greeks were the first of the peoples of South-Eastern Europe to develop a national movement of a recognizably modern type.” That movement took form in the half-century preceding the Greek Revolution of 1821, and this book attempts to display its manifold aspects. It will certainly appeal to those interested in modern Hellenism, but it also merits attention from those interested in other national movements, particularly subsequent ones within the Ottoman orbit.

The collection contains fifty-eight items, most of them excerpts from already published but widely scattered sources. Slightly more than a fourth are from the writings of British and French observers but most are from Greek sources, all but one of which (along with four French excerpts) Clogg has translated into English for the first time. Five items have hitherto been unpublished.

The collection is organized by theme into twelve

chapters, grouped into two parts. In part one (“Greek Society before Independence”) the early chapters focus on major social groups, a long one deals with the cultural revival and reaction against it, two pieces of social criticism constitute the seventh, and the eighth culminates in autobiographical sketches of Korais and Kapodistrias, each giving voice to the peaceful strategy of national liberation through education and moral regeneration. Part two (“Revolutionary Crosscurrents”) portrays the alternative strategy of conspiracy, armed struggle, and national self-reliance. Its major items are excerpts from Rigas, Kolokotronis, and Xanthos.

Like the best of its genre, this collection captures the salient features of the period/subject and enriches them with flavor and detail. As a welcome corrective to the usual stress of Western writing about modern Greece, it emphasizes the internal dynamics rather than the international aspects of the movement, and within that focus, the socioeconomic and intellectual rather than the political and administrative. But it suffers from editorial weaknesses: titles of Greek and French documents are not translated for those who do not read these languages, and the glossary does not indicate word variations (including plural forms) appearing in the text. Absence of an introduction for each document means that its author's identity and its “history” are either relegated to the general introduction—where they are sometimes too sketchy and never easy to locate—or barely indicated in the excerpt's editorial or actual title. Consequently, the untutored reader is insufficiently equipped to make allowances for personal bias, partisan debate, and special circumstances. The introduction, though excellent, might go further in relating the collection to the controversial issues of general interpretation.

These weaknesses need not detract from the lasting value of a splendid collection. They can easily be corrected in a second edition. Meanwhile, even the untutored reader or teacher can derive great benefit from the volume as is or supplement it with two collections of interpretive essays: *The Struggle for Greek Independence* (1973), also edited by Clogg, and *The First Greek War of Liberation* (1976), edited by N. Diamandouros *et al.*

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VLADIMIR DEDIJER *et al.* *History of Yugoslavia*. Translated by KORDIJA KVEDER. New York: McGraw-Hill. 1974. Pp. ix, 752. \$20.00.

Imagine the problem of writing the history of a country as varied as Yugoslavia. Even to list the

cultural, religious, ethnic, political, and other factors that have had an important impact on the Yugoslav peoples would take up more space than is allotted to this review. It should not be too great a surprise, therefore, that this single-volume history of Yugoslavia is the first published in that country, or that it has provoked bitter controversy there. The controversy concerns, among other things, the accuracy of Vladimir Dedijer's portrayal of the Partisan struggle during World War II and the alleged slights of the Croatian past by Milorad Ekmečić. But for the English language reader, for whom this translation is intended, it is much more important that we now have a relatively balanced history than that this history might be flawed in the eyes of Yugoslav scholastics.

The book is divided into four sections, each written by a specialist. The first two parts are written by Ivan Božić and Sima Ćirković and cover the period until about 1800. This one-third of the book may be the most useful for students, since studies of Yugoslav history before the nineteenth century are not common in any language and are almost nonexistent in English. Despite the occasional difficulty of the text, which requires a certain amount of concentration to follow, Ćirković's history of the medieval period is particularly good.

The spottiest section may be Milorad Ekmečić's discussion of the nineteenth century, which suffers from a none too subtle interpretation of Serbian national development and the erroneous determinist view that prior to 1903 "the trend of the national movements of the Yugoslav people was toward unification" (p. 251). Since Ekmečić is one of Yugoslavia's finest historians, however, he is able to balance these shortcomings with some excellent analysis of specific points. He comments, for example, that in Austria "the tottering aristocracy and the entire ruling class labored after 1849 under the illusion that it was they who had liberated the people from feudal bonds, and not vice versa" (p. 358).

Vladimir Dedijer's portion on the twentieth century is a three-hundred-page book in its own right. Since it provides one of the few places where the English-language reader can find a comprehensive statement of the Yugoslav view of their own revolution, it is worth reading. Dedijer pulls some punches, as in not mentioning that the negotiations between the Partisans and Germans in March 1943 resulted in a de facto, if short-lived, truce; but he is candid elsewhere, as when he discusses the erosion of the original Partisan ethic as the war progressed.

Specialists will surely find much to quibble about in this book, and naturally it is written from a Marxist viewpoint. Anyone who has read a Soviet history of this type, however, will find the

Yugoslav touch light-handed indeed. The book contains a useful index and fifty-eight good maps. At the moment, and it appears the moment may last quite some time, this is the best comprehensive history of the peoples of Yugoslavia available.

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MILOVAN DJILAS. *Wartime*. Translated by MICHAEL B. PETROVICH. New York: Harcourt Brace Jovanovich. 1977. Pp. x, 470. \$14.95.

With this, the anxiously awaited third volume of his memoirs, Milovan Djilas continues the reflective assessment of his life and the Yugoslavia which he helped bring into being through the years 1941-46. Djilas initiated his extraordinary autobiography with *Land without Justice* (1958), a stirring lyrical account strongly reminiscent of heroic Serbian epic poetry, which related his childhood and adolescence within the framework of his Montenegrin heritage. His radicalization, conversion to Marxism-Leninism-Stalinism, and rapid rise in the Yugoslav Communist Party under Tito were detailed in *Memoir of a Revolutionary* (1973). *Wartime* picks up the mounting tempo of the first two installments and presents a principal participant's view of many of the significant events and personages of the Yugoslav Revolution during World War II.

Once the Partisan struggle commenced, Djilas, as a Politburo member from the region, was put in charge of the Montenegrin theater, a post to which he was to return on several occasions during the war. He also served as an agitprop specialist and editor of the party newspapers *Borba* and *Nova Jugoslavija* and as a Partisan lieutenant general and diplomat. This book provides an invaluable insider's perspective of the legendary mountain guerrilla warfare, especially during the vividly described, crucial Fourth and Fifth Fascist Offensives in 1943, the political formation of the new Yugoslavia at Užice and Jajce, the bittersweet liberation of Belgrade, and Djilas' first two encounters with Stalin during missions to Moscow which were more completely elaborated upon in his *Conversations with Stalin* (1962). Noticeable too in *Wartime* are the first real sources and symptoms of the gradual disillusionment that later led to dissent and disaster for Djilas.

Despite Djilas' direct involvement at the highest level, his treatment of the events and the people who shared in them is accurate and surprisingly unbiased. At the outset Djilas confirms that, although Yugoslavia fell to the fascists almost four months before the German invasion of the Soviet Union and the formal dissolution of the Nazi-Sov-

viet Pact, the Yugoslav Communists planned, but carried out no organized armed resistance until June 1941, and then only after they received the directives to do so from Moscow. He is equally candid about the Partisans' negotiations with the Germans prior to the opening of the Fifth Fascist Offensive in 1943. Djilas also offers numerous intimate, revealing, but not uncritical portraits of Partisan leaders like Tito, Kardelj, Ranković, Pijade, Milutinović, Vukmanović-Tempo, Ribar, and others, and he writes freely about his deteriorating marriage to Mitra Mitrović; he generally is more faultfinding with himself than with others. Yet, while Djilas amazingly seems to hold no grudges against his fellow Partisan leaders who later turned against him, throughout the book he naively questions why most others with whom he was close during the war also denied him after his fall in 1954.

Wartime is more massive and concentrated and better documented than the preceding two volumes of Djilas' memoirs or *Conversations with Stalin*, and it is more guarded. Djilas is chronicling the Yugoslav Revolution, and notwithstanding the loss of his faith in Communism and the persecution and humiliation which he has suffered, he remains a Yugoslav patriot and unashamed of his part in the great modern national epic.

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DENNISON RUSINOW. *The Yugoslav Experiment, 1948-1974*. Berkeley and Los Angeles: University of California Press, for the Royal Institute of International Affairs, London. 1977. Pp. xxi, 410. \$16.50.

There are several general studies of Yugoslavia that competently cover the nation's history up through the 1960s or early 1970s. But this book tops them all since it offers the most thorough historical, political, economic, sociological, and ideological analysis of Yugoslavia from 1948-74. It is one of the rare books that not only corrects official Yugoslav historical inaccuracies but also sheds light on what prospects face Yugoslavia on the eve of Tito's departure as the indisputable leader and self-willed, enlightened dictator. Dennison Rusinow's study also fills the chronological gap left by another extraordinarily objective and well documented study, Walter R. Roberts' *Tito, Mihailović and the Allies, 1941-1945*. It also summarizes and reconfirms the objective, more detailed accounts of what happened during that period in Yugoslavia in the recent writings of important participants like Milovan Djilas, Vladimir Dedijer, and Svetozar Vukmanović-Tempo.

With the eye of a historian and the skill of a social scientist, Rusinow masterfully collects, sorts, and brilliantly analyzes almost every important internal and external development concerning Yugoslavia from 1948 through 1974. Many contradictory stories or rumors that were merely alluded to or omitted in previous studies of Yugoslavia are here recorded with authenticity. For both Western and Yugoslav readers *The Yugoslav Experiment* clarifies several puzzling turnabouts by Tito and the League of Communists of Yugoslavia (LCY). The study also touches upon the rise and fall of numerous important Communist leaders, intellectuals, and artists in Tito's Yugoslavia. Only Djilas' three-volume memoir reveals in greater depth the long tragic history of many of Yugoslavia's revolutionary personalities.

In eight clearly organized chapters, the trauma of the Tito-Stalin break and its consequences that pushed Yugoslavia to seek its own independent path to socialism are graphically depicted. The study confirms that the Yugoslav experiment is of great significance for non-aligned as well as Communist or Western countries. It brings into focus the strengths and weaknesses of a one party system that borrows from East and West in an attempt to reconcile many nationalities and to balance rich and poor regions in a minimally stable, cohesive, peacefully functioning federal state. Rusinow writes a book that sometimes reads like a James Bond thriller and, at other times, like a Dostoyevskian novel portraying revolutionaries at work (only this time in power). Most important, he presents the reader with an instructive, well-written chronology of the alternating successes and crises of Yugoslavia, and he analyzes the complexity of their causes. In sixty-one pages of notes, a selected bibliography, and an index, Rusinow not only reveals the depth of his understanding of Yugoslavia but he provides almost enough material for another book.

In surveying Yugoslavia's policy vis-à-vis the two superpowers, Rusinow brings into sharp focus the continuous Soviet tendency to draw Yugoslavia into its orbit. The most open and recent Soviet attempt to stir anti-Tito forces and "preserve socialism in Yugoslavia" occurred in 1973. But, as Rusinow notes, Brezhnev's desire to get a firmer grip on Yugoslavia emerged as early as 1971. The occasion was the nationalist unrest in Croatia. Tito himself "had told the Croats at the Brioni meeting in April that Brezhnev had telephoned [him] to offer Soviet 'fraternal assistance' if he should need it" (p. 299) in dealing with the rebellious Croatian Party leadership.

The book discusses hundreds of macabre developments in Yugoslavia since 1948. These well documented cases often reveal the most sophisti-

cated techniques of conflict resolution practices in the Communist world, and they increase our understanding of the psycho-political maneuvering under Communism. Perhaps inadvertently, however, the author omits the most revealing event depicting the Communist mind at work—Tito's decision to choose between freedom or oppression as an option for a nation. This dramatic choice for Tito, the lifelong Communist, occurred during the interregnum of the 1956 Hungarian Revolution. Rusinow reports the little known fact that "in September [1956] Khrushchev came secretly to Brioni to consult Tito about developments in Hungary and took Tito back to Yalta with him to meet and approve a new Hungarian leadership" (p. 90). But Rusinow fails to note an even more important move by Khrushchev in the midst of the Hungarian Revolution: he made a second secret trip to Brioni to consult with Tito. This time Khrushchev wanted Tito's opinion on whether the Red Army should re-enter Hungary to crush Nagy's democratic experiment. According to Vukmanović's memoirs (publ. 1971) most of the Yugoslav Central Committee members were in favor of greater democratization and independence for Hungary; Tito alone strongly favored and "gave approval for the intervention of Soviet troops in order to prevent counterrevolution" in Hungary (vol. 2, p. 276). What this move by Tito shows is that he preferred a Soviet-dominated Hungary to a democratic Hungary. A reason for such a gross contradiction of Tito's "non-alignment" policy was, perhaps, that Soviet domination of Hungary was thought to serve the national self-interest of Yugoslavia. A democratic Hungary would be a great threat to half-free Yugoslavia, but a Soviet-dominated Hungary would make Yugoslavia's "freedom" look better and encourage Yugoslavs to be satisfied with their lot. Another reason for such a callous attitude by Tito is that, as a life-long Communist, he preferred Soviet one-party Communism to a multi-party Western democratic Hungary. Yet, as a real national Communist statesman, Tito continues to favor such a solution only for his neighbors, not for Yugoslavia.

The book presents an orderly and well-documented story of the Yugoslav search for a socialist path that would lead to a theoretical as well as a practical alternative to the Soviet model. The author rightly refuses to prophesy whether pluralism or greater centralization will prevail in Yugoslav socioeconomic life. He clearly states, however, that Yugoslavia is and remains a mixture of the Western and Eastern ways of life.

What I might stress more strongly about the Yugoslav socialist model is that despite the fact that it was first an attempt to create a theoretical and practical way of life in response to Soviet

charges that Tito had abandoned Communism, it gradually achieved momentum of its own, and, despite many contradictions within the model, evolved further away from the Soviet pattern. It is also true that even Tito and the LCY fail to perceive all the "undesirable forces" that are by-products of that strange mixture of Marxism-Leninism, neo-Stalinism, Marxist humanism, corporate fascism, national Communism, and anarcho-syndicalism. Thus, in the multinational state of Yugoslavia it often happened that Tito and the LCY relaxed their hold, and consequently those in charge of implementing decisions from the top deviated in the direction of greater freedom and dynamism than Tito and the LCY leaders wanted to tolerate. The Yugoslav experiment proves again that even a genuine victorious revolution slides after a while into mediocrity and that even in closed societies, or in half-closed ones like Yugoslavia, not all persons or socioeconomic processes can be controlled from above. It also proves that forces of freedom lurk behind even the most dogmatic party pronouncements encouraging middle-rank power manipulators to semi-independence if not total independence.

Rusinow's book should become a standard reference work on Yugoslavia. It should be read carefully by those who want to understand the genuine desire of Yugoslav Communists to remain in power and to navigate the stormy waters of middle-eastern and Balkan power politics, trying to balance Yugoslav dependence with independence vis-à-vis two superpowers. The primary goals that the present Yugoslav leaders desire are to preserve independence, to achieve a limited degree of freedom, and, at the same time, to enable the LCY to retain the maximum control in a tensely multinational, unevenly developed, culturally and religiously fragmented country. It is a task for a divinity in our world full of nationalistic revivalism. But perhaps the Yugoslavs will pull it off; the prospect of being a Soviet colony is even bleaker for all of them.

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ILIE CORFUS. *L'agriculture en Valachie depuis la Révolution de 1848 jusqu'à la Réforme de 1864*. Translated by RADU CREȚEANU. (Bibliotheca Historica Romaniae, Section d'Histoire Économique, Études, number 53.) Bucharest: Editura Academiei Republicii Socialiste România. 1976. Pp. 215. 10 L.

VASILE NEAMȚU. *La technique de la production céréalière en Valachie et en Moldavie jusqu'au XVIII^e siècle*. Translated by ANTON MARIN. (Bibliotheca Historica Romaniae, Section d'Histoire Économique,

Études, number 52.) Bucharest: Editura Academiei Republicii Socialiste România, 1975. Pp. 269. 12 L.

Vasile Neamtu's book, the first of its kind, sets forth three ambitious objectives. He wishes to elaborate in some detail the principal characteristics of the techniques of grain production in Muntenia and Moldavia from earliest times to the eighteenth century. Secondly, the book summarizes a wide variety of work, much of it previously unpublished, on the subject. Finally, Neamtu proposes to demonstrate the essential continuity of agricultural tools, practices, and crops over two millennia among the population of the region. Since direct documentary material in this domain is not very abundant, the author employs a multidisciplinary approach, making use of work from archeology, iconography, ethnography, linguistics, etc. While the departure from a traditional historical approach suggests a broadening of the horizons of Romanian historians, it also must be recognized that the inferences from this broader approach are much more subject to debate and future clarification.

The organization of the book is straightforward. The first chapter is a thorough bibliographical essay covering both previous work on the subject and collateral research relating to his theme. This is followed by a brief but useful treatment of the physical and human geography of the region over time. The main conclusions drawn emphasize the geographical unity of Romanian territory and the persistence of an autochthonous population, conclusions that will startle no one. The remainder of the book is mainly devoted to extensive chapters on agricultural tools, varieties of grain, and agricultural practices. The emphasis here is again on the continuity of agricultural implements, products, and procedures. This does not imply a centuries-long stagnation—the complete absence of development or borrowings—but rather that the basic features essentially continued. The author further concludes that, though Romanian agriculture was ripe for a significant transformation by the medieval era, Ottoman domination not only prevented it but led to a decline.

In the main, Neamtu argues his case well and reasonably. As a treatment of agricultural practices in premodern Romania, it is an excellent reference. The ancillary conclusions regarding Romanian continuity are more debatable, but their ingenuity is noteworthy.

The Corfus book is a sequel to his 1969 work on Muntenian agriculture in the first half of the nineteenth century (*AHR*, 79 [1974]: 1592) and is the first to comprehensively analyze the critical period between the Revolution of 1848 and the epochal

agrarian reform of Prince Cuza in 1864. In contrast to Neamtu's emphasis on technical aspects, Ilie Corfus is interested mainly in the social side of Romanian agrarian history. Though this interest is the traditional one among Romanian scholars, Corfus' work differs by concentrating on detailed archival study where his predecessors focussed on juridical and parliamentary sources. As in his previous work, this study furnishes a welcome testing and correction of traditional hypotheses.

Among the questions examined are the following: 1.) How was the Revolution of 1848 accomplished in the countryside and what were its chief effects? 2.) Was corvée destroyed by the revolt of 1848? 3.) How did the subsequent re-established prerevolutionary agrarian regime evolve (especially in relation to the Știrbei law of 1851)? 4.) What resistance was there to the agrarian regime and how much of the law came into actual practice? 5.) What developments in agriculture took place in this period?

With the caution that there are important statistical and documentary lacunae, Corfus presents these conclusions: 1.) Though the Revolution of 1848 was unsuccessful and the revolutionaries' position on agrarian issues was ambiguous, the events significantly weakened the pre-1848 pattern of agrarian relationships. 2.) The thesis that corvée became a dead letter after 1848 is rejected; however, much of the corvée exacted was illegal. 3.) The Știrbei law of 1851 is seen as a key step toward contractual ("capitalist") relations between peasant and landholder, though on the surface not very progressive. 4.) Despite considerable pressure from landholders, the dependent peasantry was never completely expropriated. And, because of peasant resistance and actual revolt, many laws which seemed to give the proprietor classes the upper hand could not be enforced in most regions. 5.) Agricultural development remained basically backward, with some scattered advances. The area under cultivation underwent considerable expansion, doubling or tripling in some regions in comparison with the first half of the century. Harvests, on the other hand, were not commensurate with this increase, being in general poor. 6.) Finally, the period saw a substantial social transformation in the villages with the development of a wealthy peasant class.

Corfus modestly concludes with the hope that his study will provide points of departure for further work in this "arid domain." It seems more likely that, except for certain broad interpretive issues, his excellent book makes further work in this area unnecessary for the foreseeable future.

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RUMIANA RADKOVA. *Neofit Rilski i novobŭlgarskata kultura: Pŭrvata polovina na XIX vek* [Neofit Rilski and Modern Bulgarian Culture: First Half of the Nineteenth Century]. Summary in English. Sofia: Izdatelstvo nauka i izkustvo. 1975. Pp. 227. 1.46 lv.

Neofit Rilski was the foremost educator in Bulgaria in the late 1830s and 40s. Although he was not the first to establish a secular school, Rilski was the first to introduce modern methods of teaching and to make Bulgarian the language of instruction. Through his efforts and those of his students the old traditional church schools were transformed into modern institutions. Rilski made the Gabrovo and Koprivshtitsa schools and the Bell-Lancaster method of teaching models for other schools to follow. As Rumiana Radkova correctly notes, through his teaching and writings Rilski contributed not only toward the education of hundreds of Bulgarians but also to the strengthening of Bulgarian national consciousness, the formation of an intelligentsia, and the creation of modern Bulgarian culture.

The lack of materials for certain periods of Rilski's life makes it difficult to write a good biography of "the patriarch of Bulgarian writers and educators," as K. Jireček referred to him. To determine the character of his activity and to evaluate Rilski's contribution to Bulgarian national development, Radkova therefore relates his teaching and writing to the various social and cultural trends of the period. She examines in detail Rilski's share in the formation of the modern Bulgarian literary language, his translation of the Bible into modern Bulgarian, his constant concern for and support of his former students, his life-long work on the Greek-Bulgarian and Bulgarian-Greek dictionaries, and his contacts with Bulgarian intellectuals abroad. According to the author the monk from Rila represented the interests of the Bulgarian bourgeoisie, and most of his shortcomings—like his religiosity and loyalty to the Orthodox Church—are attributed to Bulgarian-Ottoman backwardness.

There are a number of questions for which Radkova does not provide answers. For example, a comparison between Neofit Rilski and Neofit Bozveli would have been helpful in providing some clues as to why the former did not become directly involved in the struggle for a Bulgarian national church and the political liberation of Bulgaria. An examination of the careers of Rilski's students might supply answers to his influence among the Bulgarians. Since the latter part of Rilski's life is not as fascinating and even as significant as his earlier years, and because in all likelihood no one will write another book on Rilski in the near fu-

ture, a treatment of the whole of Rilski's life would have made this a much more valuable work.

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LÁSZLÓ DEME. *The Radical Left in the Hungarian Revolution of 1848*. (East European Monographs, number 19.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1976. Pp. x, 162. \$12.00.

Three times in 1848 Hungarian radicals of the Left influenced the trend of revolutionary events. In March and April a group of intellectuals and university students representing "Young Hungary" and led by the great poet Sándor Petőfi, his close friend, the novelist Mór Jókai, the historian Pál Vasvári, and the journalist József Irinyi resorted to direct popular action in the new capital, Pest. "Twelve Points" were formulated as the Hungarian nation's demands; they reflected the egalitarian spirit of the French Revolution and the individualistic as well as constitutional goals of contemporary liberal nationalism. As a result, the last feudal Diet sitting in Pozsony (Bratislava, Pressburg) at the time of the fall of Metternich passed a series of reform acts which abolished feudalism and established an independent and responsible parliamentary government for Hungary. Sanctioned by the Emperor-King Ferdinand according to Hungarian public law, the work of the liberal government of Lajos Batthyány, which included the country's best minds—men such as István Széchenyi, Ferenc Deák, József Eötvös, and Lajos Kossuth—was scrutinized by a Committee of Public Safety formed by the radicals to safeguard the achievements of the March Revolution.

These conditions of "dual power," a term borrowed by the author perhaps with a measure of exaggeration, changed when the first popular parliament convened in Pest in early July. The radical literati did not fare well in the elections: only two of them, Irinyi and the lawyer Dániel Irányi, won mandates. Yet, under the leadership of Pál Nyári, the Madarász brothers, László and József, as well as the future general Mór Perczel, some 50 out of 300 deputies organized a bloc of representatives in the National Assembly which constituted the second major component of Hungarian radicalism. Unlike their youthful plebeian and often republican counterparts, most members of this parliamentary group of radicals came from the landowning lesser nobility, belonged to the liberal opposition before 1848, and had experience in the county administration. Like their literary com-

rades in arms, they too formed their progressive clubs, such as the Society for Equality, and published papers agitating for radical reform. Although defeated in the elections, the radicals gained strength in July and August during the debate over the establishment of a new Hungarian army and aid to Austria in the war against the Italian national revolution. Asserting Hungary's rights, Kossuth, who in the spring spoke of crushing the young rebels, relied increasingly on the radicals, who in turn tried to detach him from the majority of more conservative cabinet members seeking cooperation with Vienna at any price. The stiffening of the Austrian attitude after her military victories in Italy, the concomitant attack of the Croatian frontier regiments, and the insurrection of Serbians and Romanians against a Hungarian government reluctant to make further concessions on the interlocking questions of the peasantry and the nationalities led to the collapse of Batthyány's ministry and the creation of a Committee of National Defense headed by Kossuth. Yet placing the government in the hands of the Committee dominated by the radical minority in reality meant the transfer of dictatorial power to its president, "Citizen and Member of the Assembly, Kossuth."

Sober and scholarly, László Deme's analysis of the social background, ideological inspiration, and activities of the Hungarian radicals is a useful contribution to the growing English literature on the revolutions of 1848. Although they failed to identify as completely with the special interests of the incipient working class, or even the liberated peasantry, as did the self-educated teacher and labor leader Mihály Táncsics, the radicals stood for democratic principles pointing toward a political and social egalitarianism which went beyond the immediate goals of the majority of the liberal nobility. They advocated the full emancipation of Jews and an alliance of all, Magyar and non-Magyar, nationalities of Hungary in an anti-Habsburg revolutionary struggle for independent statehood.

Like the radical Count László Teleki, whose intellectual honesty the author duly stresses, in his negotiations with Polish and other exiles in France, Baron Lajos Splényi, the Hungarian revolutionary government's envoy first in Turin, and later in Constantinople, also promoted the radicals' progressive views on the nationality question. This would seem to justify assigning greater weight to the evaluation of the accomplishments of Hungarian radicalism in the light of the international situation than László Deme is prepared to do (p. vii). Whether or not the *concept* of national self-determination was spelled out *expressis verbis* in

the mid-nineteenth century, pertinent demands of non-Magyars, as this study proves (pp. 67-72), were formulated with sufficient emphasis, and the radicals recognized this. The attack of the Croatian troops under Baron Joseph Jelačić, too, had multiple domestic and international ramifications, as shown by Gunther E. Rothenberg's recent studies.

The author's due emphasis on the influence of the "great" French Revolution on Hungarian radicalism (pp. vii, 17, 78) would have benefitted by a few references to the impact of American revolutionary ideas in the pre-March period, a topic barely mentioned. Deme is right in arguing that the radicals were indispensable to the victory of the Hungarian revolution in 1848, but his hypothesis about the imminence of a "second revolution" in September (pp. 96-99, 147-48), despite Petöfi's talk about (and Széchenyi's fear of) it appears unconvincing in the absence of more tangible evidence; the Hungarian Marxist authority mentioned by him, György Spira, refutes the theory in his more recent pertinent monographs (1964 and 1972).

The text suffers from an excessive number of printing errors which careful proofreading could have eliminated (e.g., pp. 112-13, 117). Indeed the usefulness of this informative study could have been further enhanced by including in the bibliography relevant major monographs in English such as Rothenberg's on the Croatian Military Border, Spira's on Széchenyi, and Király's on Deák.

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JUDIT KUBINSZKY. *Politikai Antiszemizmus Magyarországon, 1875-1890*. [Political Anti-Semitism in Hungary]. Budapest: Kossuth Könyvkiadó. 1976. Pp. 271.

In Germany, Austria, Romania, and Russia, anti-Semitism became a permanent feature of the political landscape after 1880; in Hungary this was not so. Despite the Tiszaeszlár blood-libel affair of 1882-83 and the establishment of a flamboyant anti-Semitic party in 1883, Saint Stephen's kingdom remained for thirty more years so free of overt prejudice against Jews that the mayor of Vienna labeled Budapest "Judapest." Most historians have attributed this philo-Semite record solely to a deliberate policy of the Magyar nobility, which required help from Jewish capitalists to preserve its dominion over Greater Hungary's nationalities and landless peasantry. Judit Kubinszky's well-documented monograph suggests an explanation more in terms of Hungary's retarded social development.

Kubinszky studies in detail the anti-Semitic propaganda of the 1870s and early 1880s in Hungary, the police reports on Tiszaeszlár and the pogroms of 1883, and data about Győző Istóczy's election campaign in 1883–84. She finds first a striking disproportion between Istóczy's propaganda voice, which was capable of drawing international attention, and the weakness of his organization, which was unable to challenge the existing political parties. She attributes this weakness to a lack of potential followers. Since Hungary did not yet have an extensive urban petty bourgeoisie, or a large student population, anti-Semitism could not develop as in Vienna and Berlin, but had to fall back on dispossessed petty nobles, county officials, village priests, and the like. Even of these, she indicates, there were enough only in a band of pure-Magyar counties along the south of Lake Balaton and east of Budapest where enormous estates, rented to Jews, were adjacent to economically pressed small-holdings. Only after 1900, when the Budapest bourgeoisie became stratified, the great magnates abandoned liberalism, and the dispossession of the middle-nobility became massive, did the social base for political anti-Semitism emerge in Hungary.

Kubinszky neglects an important ethnic factor: in the 1880s, as in the past and as in 1919, anti-Semitism flourished among the German burghers of the West Hungarian towns and even Pest. They gave Istóczy's movement an "imported from Germany" tint which inhibited Magyar nationalists from joining him. But this omission does not detract from the importance of the book in underlining the complexity of the European anti-Semite outburst of the 1880s. There was not just one wave, as has often been suggested, but two—the first popular, thriving in those East Elbian lands where an urban petty bourgeoisie had already emerged, the second state-inspired, thriving in agrarian Russia and Romania. In between there were semi-developed lands where, in the absence of state support, this sort of modern movement could not yet prosper.

The book is effectively written and contains a bibliography and index, but has no summary for persons who cannot read Magyar.

WILLIAM O. MCCAGG, JR.
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GYULA JUHÁSZ. *Magyarország külpolitikája, 1919/1945* [The Foreign Policy of Hungary, 1919/1945]. 2d ed. Budapest: Kossuth Könyvkiadó. 1975. Pp. 434. 50 Ft.

Gyula Juhász, a member of the prestigious Institute of History in Budapest, knows contemporary

Hungarian diplomatic history better than anyone. Although he has written a popular work without footnotes, his is an original study, enriched in its second edition with the author's recent findings in the British Public Record Office and other places. There are too many small details for a truly popular work but there are also some admirable summaries dispersed in the text. The story begins with the destruction of the old order, at the end of World War I, and the subsequent failure of democrats and Communists to create viable alternatives. These failures the author attributes to the hostility of the Entente politicians in Paris. Unlike Mihály Károlyi and Béla Kun, the counter-revolutionary Admiral Horthy received Entente—especially British—support in 1919, and thus the new regime did not start out in complete diplomatic isolation. But British interest was sporadic, as was French interest, while Hungary's new neighbors were consistently hostile and suspicious. The Horthy regime made the integral reconstitution of historic Great Hungary the sole aim of its diplomacy; this decision, dictated mainly by domestic considerations, drove Hungary toward any power that was likely to secure territorial revision without, at the same time, endangering the country's atavistic social structure. Thus rapprochement with equally revisionist Soviet Russia was impossible, as was, at first, rapprochement with democratic Austria or Germany.

The Hungarians, especially Count István Bethlen in the 1920s, were not unsuccessful in breaking out from their diplomatic isolation. By 1932 remarkable progress had been made in improving Hungary's international position but then the Great Depression and the consequent annulment of Western loans ruptured Hungary's weak ties with the great democracies. As fascist Italy—Hungary's self-appointed protector—increasingly proved itself unreliable and useless, the perpetual quest for territorial revision and for economic advantage inevitably led to ever closer Hungarian ties with Nazi Germany. Yet alliance with Hitler was never unconditional: the Hungarians were as fearful of German imperialism, and of the revolutionary potential of the Nazi message, as they were anxious to secure German political and economic aid. But while some territories were eventually regained, during World War II, Hungarian independence was lost to Germany.

Did Hungary have a choice in all this? The author says yes repeatedly, only to point out in the same breath that there was in fact no choice. He points out, for instance, that in order to reconcile the Little Entente in the 1920s, Hungary would have had to undergo a democratic transformation, as well as abandon the goal of integral territorial revision. But then he shows that the Horthy re-

game was incapable of democratic transformation and territorial renunciation: nor were there any domestic forces able to overthrow Horthy. As for Hungary's neighbors, they were unwilling to return as much as a village to Hungary. Or, to give another example, Juhász insists that during World War II Hungary should have turned against Germany. But then he demonstrates convincingly that armed struggle against Germany was impossible because of the lack of arms (weapons could be secured only from Germany), because of the violent hostility of Germany's other satellites such as Romania or Slovakia, because of the strength of the domestic fascists and the paralysis of the anti-fascists, and last but not least, because of the physical absence and the indifference of the Western Allies. He also admits that armed resistance would have led to the early deportation—and hence the total extinction—of Hungary's one million Jews. Juhász's progressive beliefs forbid him to admit that at least some of Hungary's wartime leaders, such as Pál Teleki or Miklós Kállay, did their best to save the country, yet he is fair enough to make it clear that others would not have done better. The result is a series of logical contradictions, none of which detracts from the fundamental value of this fine book.

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BILL LOMAX. *Hungary 1956*. New York: St. Martin's Press. 1977. Pp. 222. \$14.95.

Although many books have appeared on the Hungarian Revolution in the last twenty-one years, the topic is far from exhausted. A number of questions about this major rift in the socialist camp have remained unanswered, or answered in an unacceptably biased or superficial manner. Bill Lomax attempts to answer as many of these questions as possible within the limits of a slim volume, and by and large he succeeds in retaining the clinically sober objectivity so necessary when examining an event of historical proximity and great controversy.

Lomax's examination of political diversity in Hungary begins with the year of Stalin's death. (To treat the pre-1953 events would have been beneficial, as it would have provided an insight into the make-up and activities of the political parties which were revived in 1956.) Lomax succinctly describes the opposition's ideology, or the lack of it, and the haphazardly brutal manner in which the Rakosi clique attempted to retain power.

A persistent question raised by scholars of the period concerns the relative role played by the

various social classes in the events of the Hungarian October. Lomax's central theme is that the Hungarian intellectuals and politicians, to whom he refers jointly as the "elite," were less responsible for the birth and development of the revolution than the working masses. While this claim has considerable merit, the account of events supports it only partially. The Hungarian "elite" was far from monolithic and became even less so after 1953. In those years its two mainstays became increasingly more opposed to each other. The politicians, Moscow-trained and native alike, felt protected by the status quo, the centralized one-party system, and the presence of Soviet troops; they wanted to bring about only minor reforms, mostly of an economic nature. The intellectuals desired and worked for more basic changes; they felt that they deserved to be considered free and equal partners in the new society, and that it was time to relax the enforced discipline which was declared necessary during the internal struggles of the transformation years. They felt threatened, both as public figures and as creative individuals, by the continuation of Stalinist policies.

Lomax's stress on the discontent existing among the workers and the peasants is entirely valid, as is his recognition of the role played by their organizations, the workers' councils, during and after the revolution. It seems unlikely, however, that the popular dissatisfaction would have brought about more than chaotic and useless strikes, passive resistance, and an occasional flare-up of violence. The workers' desire for improvement and their readiness for action were reinforced and modified by infusion with the intellectuals' demands for controlled but thorough changes, and this gave the revolution its basic character. Traditional Hungarian nationalism had an important part in the events of October, but, again as a result of the influence of the intellectuals, all demonstrations and proclamations remained remarkably free of chauvinism or revanchism, and even anti-Russian sentiments were subdued until the attack on the fourth of November.

Possibly because of limitations of space and time, another thorny issue, that of Western "complicity" in the Hungarian Revolution, remains to be treated satisfactorily. The numerous references to the incendiary effects of Western broadcasts and propaganda announcements are undocumented by footnotes. Even in the present age of Western breast-beating, this lessens their credibility. Similarly, lengthy accounts of Western agents and armed "fascist exiles" entering Hungary remain entirely unsupported by hard data. The book seems to founder in its examination of the working classes' social and political characteristics, and this points out anew the necessity for

Western sociologists, and specifically labor historians, to do more exhaustive research on this critical group in the socialist countries. Finally, a minor, yet always disturbing, deficiency is the total lack of diacritical marks, which can cause serious misunderstanding in a book dealing with Hungary.

Certain shortcomings aside, the volume is as well researched as possible without access to Hungarian sources. It is written in a lively, journalistic manner, and is equipped with a chronological chart and several maps. While it should not be considered the definitive monograph on the Hungarian Revolution, it is a valuable addition to the library on the topic.

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PAUL VYŠNÝ. *Neo-Slavism and the Czechs, 1898-1914*. (Soviet and East European Studies.) New York: Cambridge University Press. 1977. Pp. xiv, 287. \$21.95.

The short-lived Neo-Slav movement has long attracted historians seeking to explain why efforts to promote economic and cultural cooperation among the Slavs of Austria-Hungary, the Balkans, and the Russian Empire should have originated early in the twentieth century and expired with barely a spasm before World War I. Rather than offering any novel explanations, Paul Vyšný stresses the hopeful liberalism that briefly flowered after the Russian Revolution of 1905 and the ambitions of public men at the time of suffrage reforms in Russia and the Austrian portion of the Habsburg Empire. He illustrates these factors well by tracing the participation of Roman Dmowski, Václav Klobáček, Alexander I. Guchkov, and others who flirted with Neo-Slavism and benefitted from mention in the press. Vyšný's account of the Sofia Congress of 1910 is the fullest treatment available of the movement's swan song. By that time, the increasingly conservative complexion of the Russian delegation constituted an internal dynamic that hastened Neo-Slavism's demise.

Vyšný properly concentrates on the activities of Neo-Slavism's prime figure, the Young Czech politician Karel Kramář. He effectively uses Kramář's extensive writings and the record of his trial for treason by an Austrian military court in 1915 to show the shifting ground of his position. Kramář's early Neo-Slav optimism weakened, first because of the manner of the Austrian annexation of Bosnia in October 1908, then because of resurgent tsarist oppression in Russian Poland. By early 1914 Kramář was openly pessimistic over the chances of

any meaningful inter-Slavic cooperation. When Vyšný attributes Kramář's sponsorship of Neo-Slavism primarily to opportunism, however, he both undervalues the sentimental pro-Slavic feelings that had animated the Czech leader since his youth and ignores two decades of striving by Kramář to reorganize Austria along federal lines through the instrumentality of a Slavic majority in the Austrian parliament. This was, to be sure, an unsuccessful strategy, but its revival by Kramář under the Neo-Slav banner represented but another stage in his long quest for a reconstructed Austria. One question also Vyšný's air of finality when he counterposes to Kramář's testimony the judgments of Edvard Beneš. Beneš wrote about Neo-Slavism many years after its demise, when he was Kramář's bitterest political foe.

To accumulate his wealth of detail the author has combed the memoirs of leading Neo-Slavs, Russian and Czech newspapers and periodicals, and archives in London and Vienna. Scarcely any older study that bears on his subject has escaped his eye, but he has not used the valuable study by F. R. Bridge, *From Sadowa to Sarajevo* (1972), which might have broadened his perceptions of long-range aspects of Austrian foreign policy, and he has relied excessively upon dated English-language works for the background to Polish and South Slav affairs. The materials from Czech archives which he cites are limited to references by contemporary Czech historians in works that are often highly selective in their choice and interpretation of the sources. Notwithstanding these reservations, Vyšný has produced the most thorough study yet of Neo-Slavism.

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YESHAYAHU JELINEK. *The Parish Republic: Hlinka's Slovak People's Party, 1939-1945*. (East European Monographs, number 14) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1976. Pp. viii, 206. \$11.50.

The author is senior lecturer in history at the University of Haifa. His book deals with the history of the Hlinka party—not, as the title suggests, of the Slovak state during World War II. As is often the case with a doctoral dissertation, its value lies primarily in the discovery of previously unexplored archival materials and accumulation of facts, rather than in the depth of perception and originality of interpretation. Although archival sources in Czechoslovakia were not accessible to him, Yeshayahu Jelinek used an impressive array of documentary materials in the West.

The main sources used were the captured German records—whether in originals or on microfilm—in the U.S. National Archives and in West Germany; the Nuremberg trials records; Israeli collections such as the Moreshet Archives, Givaat Havivah, the Oral History records of the Hebrew University of Jerusalem, and the Slovak Jewry Collection of Yad Vashem, Martyrs and Heroes Memorial Authority in Jerusalem. The bulk of the documentation comes from German sources, but Slovak materials—such as Tiso's unpublished diary at the Hoover Institution—were supplemented by such official publications of documents as appeared in Czechoslovakia during the thaw of the sixties. It is interesting to note that the author met with a negative response when he attempted to plumb the Vatican archives; the Catholic Church had a great influence in the Parish Republic, but it does not care to reveal the secrets of its relations with Slovak leadership at that time.

For more than five years the Hlinka party held supremacy in Slovakia, and Catholicism was the very soul of the party and its ideology. Members of the clergy were party functionaries, and clerical pressure was evident everywhere. The Church dominated cultural and literary life, and controlled the spiritual development of the citizens, having established an especially tight grip on schools and a censorship of the printed word reminiscent of the Spanish Inquisition: even *Robinson Crusoe* was banned from libraries and replaced by accounts of the lives of saints. The priests in the party were very much of this world: they dominated the economy and did not hesitate to profit from aryanization of Jewish property. The Church became rich by land grants and donations from the state and individuals. Other organized churches were not tolerated, and Protestant sects were persecuted, while atheists had a status almost equal to that of Jews.

Aggressive nationalism was the second pillar on which the party rested. Intensive indoctrination of schoolchildren and forced Slovakization of national minorities was its primary concern; the population was divided into four national groups and only the Slovaks were first-class citizens. Germans had to be granted some privileges because they were supported by the Reich, but Magyars were underprivileged, Ukrainians and Poles oppressed, and Jews and gypsies persecuted and victimized. The Czechs, if tolerated at all, were harassed.

The third main feature of Slovakia under the Hlinka Party regime was authoritarianism. Domination of population through agencies of the state was supplemented by intervention of various front organizations, institutions, and corporations. From a mass party the Hlinka party evolved into a form of Caesaropapism, with religious and politi-

cal authority uneasily united in the hands of the president and party chairman. The fascist Hlinka Guard presented opposition on the right, but it was defeated by the clerical wing and the party became elitist, totalitarian, and dedicated to the *Vodca* (Führer) principle.

The totalitarians never managed to establish complete control. They were threatened by the Reich-supported German minority and by the Slovak army, and they suffered from lack of unifying leadership. President Tiso never gained the position of an uncontested *Vodca*, primarily because the real Führer called all the shots from Berlin. The party floundered from crisis to crisis, unable to spare the nation the disasters which befell it during the war.

This is a useful and thorough study, even if limited; it leaves the reader somewhat uncertain about the over-all events in the Slovak state in that period. The author foresaw its use by historians of East-Central Europe as well as by students of comparative politics. Perhaps this double aim accounts for his organization, which is somewhat confusing, the book consisting partially of chronological narrative, partially of an analytical presentation. The gap between the two is not bridged even in the final chapter of conclusions.

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ZDENĚK HEJZLAR. *Reformkommunismus: Zur Geschichte der Kommunistischen Partei der Tschechoslowakei*. Cologne: Europäische Verlagsanstalt. 1976. pp. 479. DM 54.

VLADIMÍR HORSKÝ. *Prag 1968: Systemveränderung und Systemverteidigung*. Stuttgart: Ernst Klett Verlag, and Kösel-Verlag, Munich. 1975. Pp. 534.

The Prague Spring of 1968 is the common theme of these two books by Czech exiles, both of whom were Communist Party members and active participants in the movement for reform. Zdeněk Hejzlar, now a research worker in international affairs in Stockholm, a former inmate of Nazi concentration camps and Stalinist prisons, and the victim of a purge in 1961, was rehabilitated only in 1968 and appointed head of the Czechoslovak radio. Vladimír Horský, now a research worker in Heidelberg, was a scholar in the Sociological Institute in the sixties and a member of the Prague municipal party committee in 1968. Both write as strong supporters of the reform movement in its relatively moderate official form and criticize what they regard as the "radicalism" of some reformers. Both believe that the reform of Communism was a genuine possibility, and both condemn without reservation the Soviet intervention which terminated it.

Theirs is therefore an immanent critique, differing, however, in the balance of positive and negative criticism of the movement.

The two books offer an interesting contrast in approach and method, although each is a substantial piece of scholarly research. Hejzlar, who was not, prior to emigration, engaged in scholarly work, follows traditional paths, employing detailed empirical data drawn from Czech sources and eschewing theory or even substantial analysis. Approximately one-third of his book is devoted to the historical background of 1968, one-third to the events of that year prior to the invasion, and one-third to the post-occupation "normalization" under Dubček and Husák.

Horský, on the other hand, is highly theoretical, seeking to answer the questions "why reform?" and "why the invasion?" within the framework of systems analysis. Omitting any substantial historical introduction, Horský devotes one-third of his book to the Prague Spring; one-third to the August intervention, the resistance, and the Moscow talks; and one-third, the most unusual, to a theoretical examination of possible alternative outcomes. His resource base is less detailed, at least in the first two parts of his book, and his treatment is often little more than a descriptive chronology of events. In the third part, however, Horský makes exhaustive use of post-1968 publications, at home and abroad, and examines every detail of interpretation, even in sources of journalistic character and dubious quality.

Horský directly confronts the question of whether military intervention could have been avoided, in view of the profound Soviet hostility to the basic transformation of the system under way. He argues that military intervention was not inevitable, and, after a balanced analysis of both "realist" and "radical" critiques of the Dubček course, concludes that the main fault lay in the weakness of Prague's foreign policy, and, in particular, in the failure to enlist the support of the Western European Communist Parties in its cause. Although Hejzlar does not explicitly address the same question, he argues that a cautious and moderate tactic was necessary, and that the words and deeds of the "maximalists" or "radicals" did not contribute to the compromise with the Soviet Union that was necessary. Neither of the two authors, be it noted, believes that preparations for armed resistance would have been wise, or that such a course was desirable at the time of the invasion.

Horský is severely critical of the post-occupation policy of the Prague leadership, especially the actions of President Svoboda, and the acceptance of the protocol at the talks in Moscow, which, in his view, was bound to lead to complete capitulation.

Hejzlar, on the other hand, criticizes the leadership not so much for signing the Moscow agreement as for failing thereafter to carry through a retreat to certain lines, and then to defend these firmly against Soviet interference. Horský regards the policy of "normalization" pursued by Dubček on his return from Moscow as more or less equivalent to surrender, whereas Hejzlar considers it as a point of departure for continuing the reform program in some degree. In the absence of the firm policy which he urged, however, he, too, believes that it could only have led to submission and ultimate catastrophe.

For historians, the most interesting feature of Horský's book is his examination of whether an alternative post-invasion strategy would have avoided this outcome and saved at least part of the reform and Czechoslovakia's independence. Employing certain theoretical propositions derived from Max Weber and Ernst Bloch concerning the absence of determinism in historical events, Horský seeks to answer the question of "what would have happened if?" He argues persuasively that nonviolent resistance was a viable alternative. The immense wave of national resistance during the first week of the invasion had already foiled Soviet plans and forced Moscow to make significant retreats. Horský believes that if this had been continued, it would have produced results more favorable to Czechoslovak interests than any alternative, including armed resistance, capitulation, or "normalization." The Soviet Union, on the basis of a sober cost-benefit calculation of its real interests, in world affairs, Eastern Europe, and the world Communist movement, would have had to make further concessions and compromises. Perhaps less convincing, but thought-provoking, is Horský's more general conclusion that such a strategy of "social defense" is suitable for other cases of small-power resistance to superpower intervention.

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JERZY TOPOLSKI, editor. *Dzieje Polski* [History of Poland]. Warsaw: PWN. 1976. Pp. 935. 300 Zł.

This is a synthesis of the entire prehistory and history of Poland. It is a collective work of eight historians, each responsible for his own part or chapter; all contributions were submitted to the sole editorship of Jerzy Topolski. The complete work is clearly based on the acceptance of the "Marxist research method," which differs from "Marxism fatalistically interpreted," as stated in the editorial introduction, but still remains the only method that, when used concomitantly with the "comparative method," is able to lead toward

an interpretation of Poland's history that is close to "the laws of history." This clearly Marxist character of the book makes it different from the multi-volume *Historia Polski* being issued by the History Institute of the Polish Academy of Sciences, or from the textbook in English, *History of Poland*, written by non-Marxist or not "sufficiently" Marxist scholars. *Dzieje Polski* also differs slightly from *Istoriia Pol'shi*, a three-volume work of the *Institut Slavianovedeniia* of the Soviet Academy of Sciences, which is more dogmatic and restrictive on some points. This Russian history of Poland, however, has remained the blueprint for the orthodox interpretation of major processes in Poland's history, apparently binding upon the authors of *Dzieje Polski* as well.

In spite of the "Marxist method of research," the material is arranged in rather classical subdivisions: "Prehistory of the Land," "Poland of the Middle Ages," "Modern Poland," with a strong emphasis on the Renaissance and Reformation, Polish Baroque, parliamentarism, the Enlightenment, the reforms articulated at the Great Diet, and the Partitions. The short period between the last partition and the Congress of Vienna constitutes a separate section, followed by "Polish Lands under the Domination of the Partitioning States, (1815-1918)," "Poland in the Twenty-Year Period between the Wars," "Poles and the Polish Question during WW II," and "Major Stages and Directions in the Development of People's Poland." The Marxist method is primarily reflected in the authors' acceptance of economic determinism and of the class struggle which, they argue, permeates the society, the nation, and the state, and which constitutes the dominant thread in a uniform presentation of the chronological flow of history. The nobility, bourgeoisie, and class of proprietors used the state to perpetuate their privileges and exploit the masses. Progressive thoughts and programs tended toward a change of relations. But only Marxism provided a basis for the proper changes; only Communism is truly progressive. All opponents of Communism are reactionaries, although *Dzieje Polski* is more subtle in its differentiation between various non-Communist programs and ideas than *Istoriia Pol'shi*.

This book offers an enormous collection of factual material. The "factographic" aspect prevails over questionable interpretations and vague or dogmatic generalizations in the sections dealing with medieval or modern Poland. But in its coverage of most recent history the book reveals enormous biases and preconceived notions; here this "factographic" material has less value. The book distorts facts and events that do not support the political interests of the Communist Party. It omits whole areas that cannot be interpreted con-

vincingly. Some examples of omission will illustrate the point. The decision to invade Poland during the Nazi campaign of 1939 had already been made in the secret codicil of the Ribbentrop-Molotov Pact of August 23, 1939. For the authors of *Dzieje Polski*, by the middle of September "the strategic situation of Poland was subject to analyses by the—still now—neutral Soviet government" (p. 774). Omitted are deportations from Eastern Poland to the Soviet Union; the massacre of Polish officers found in the mass graves of the Katyn Forest—only a portion of the 14,300 who disappeared in the Soviet Union; the treacherous arrest of the Polish Delegation of the London government; the imprisonment and deportations of soldiers of the Home Army and Peasant Battalions in 1944-45; the persecution of churches and of religion; the imprisonment of Cardinal Stefan Wyszyński; the presence of the Red Army in Poland's territory, etc. Even the events of 1956 that brought Władysław Gomułka from imprisonment to power, and those of 1970-71 that gave leadership of the party to Edward Gierek, are "neutralized" and deprived of a political or historical meaning.

The book is based on publications supporting Communist political interests. Such scholars as Stanisław Kutrzeba or Adam Vetulani are not even mentioned. No publication on Poland that has appeared in Western Europe is considered. Particularly, such scholars as Oskar Halecki, Stanisław Kot, Marian Kukiel, Henryk Paszkiewicz, and Piotr Wandycz are not mentioned nor are their publications used.

One wonders whether all this is evidence of terrible intellectual terror or of lack of intellectual integrity in this younger generation of Communist scholars.

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MACIEJ SERWAŃSKI. *Henryk III Walezy w Polsce: Stosunki polsko-francuskie w latach 1566-1576* [Henry III Valois in Poland: Polish-French Relations in the Years 1566-1576]. Summary in French. Cracow: Wydawnictwo Literackie. 1976. Pp. 294. 40 Zł.

Although Polish historiography has long displayed a strong interest in the first and second interregnums which followed Zygmunt August's death and the end of the Jagiellonian dynasty, there has been little attention devoted to the genesis of the reign of Henry Valois—who was elected king during the first interregnum—in the context of Polish-French diplomatic relations. W. Sobieski, in his work on the subject, dealt with Polish and Huguenot relations after the St. Bartholomew Massacre.

P. Skwarczynski analyzed the pre- and post-electoral negotiations of the Poles, Lithuanians, and French in 1573 regarding the Pacta Conventa to be imposed upon the elected monarchical candidate. The situation is somewhat better in French historiography, with the century-old but still useful researches of E. de Noailles and the more recent work by P. Champion who, however, viewed his subject exclusively from the viewpoint of French *raison d'état*.

Maciej Serwański utilizes the work of his predecessors—particularly of de Noailles—and of Polish and French archival sources. (Unfortunately, the author had only limited access to French archives, but this does not seriously undercut the book's scholarly value.) As the subtitle of his book indicates, Serwański deals with a broader canvas of time than the five months in 1574 which Henry Valois, heir to the French throne, spent in Poland as its newly elected monarch. His field of concern is the ten-year period during which Poland and France strove for closer ties in order to fulfill various political objectives, the most crucial being that of containing the might of the Habsburgs by keeping them from occupying the Polish throne and by coming to terms with the Turks. This period of diplomatic history began with Zygmunt August's attempts to improve relations with France, and ended with the succession to the Polish throne of Stefan Batory, which would annul France's influence in Polish affairs for decades.

At the heart of this volume is one of the more bizarre chapters of sixteenth-century European history: the election, coronation, and subsequent flight from Poland of Henry Valois. As is well known, the young monarch secretly fled from Cracow after receiving the news of the death of his brother, Charles IX. As Henry III, he was to reign in France for fifteen years, although stubbornly retaining the use of the title "King of Poland" until his death. However, all the efforts of French diplomacy, described in detail by Serwański, could not repair the impact of that "night-time flight" upon the angered Poles.

Serwański concludes his work with an impressive bibliography of source materials and secondary works.

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JERZY ŁOJEK. *Upadek Konstytucji 3 Maja: Studium Historyczne* [Collapse of the May 3d Constitution: A Historical Study]. (Polska Akademia Nauk, Instytut Badań Literackich.) Wrocław: Ossolineum. 1976. Pp. 321. 80 Zł.

This book argues that the overthrow of the reformist constitution of 3 May 1791 was not inevitable.

Jerzy Łojek contends a vigorous defense would have protected it against domestic and foreign enemies. Unfortunately the Polish government failed to act resolutely. Łojek assigns primary guilt to King Stanisław August Poniatowski who, he shows, was not personally committed to the constitution, made many concessions to its opponents, and failed to defend it against attack.

While well presented, Łojek's arguments are not very new. His book follows the argumentation of an eighteenth-century book whose title is echoed by Łojek's volume. Other elements of his presentation can be found in works written fifty or more years ago by Smoleński, Lord, and Askenazy. Łojek appears overly optimistic about Poland's chances of defending itself against Russian might. Moreover, in assigning Poniatowski major responsibility for Poland's failure, Łojek goes too far. Even material presented within this book shows that the other authors of the constitution, especially Ignacy Potocki and the Czartoryski Party, failed to support it adequately after its enactment. Poniatowski was a constitutional king, but no one stood up to challenge his policies when they were wrong.

Despite this fundamental weakness, Łojek's book is provocative and informed, stemming from years of archival study. Most of the material comes from familiar archives, but he has examined them afresh and exploited some unfamiliar corners. Some interesting new material comes from the papers of Ignacy Potocki, although more is to be found there than Łojek uncovered. The most original contribution is developed from earlier books by Łojek. He argues that Russian intervention in favor of Targowica and against the May 3 constitution was not inevitable because some powerful court figures accepted the Polish "revolution." Łojek concludes that vigorous Polish diplomacy might have forestalled such intervention. Łojek's description of the Russian scene is plausible, although the information is fragmentary, and conclusive proof is locked away from prying Western and Polish eyes alike by Soviet archivists. Here too Łojek's conclusion is overly optimistic.

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PIOTR S. WANDYCZ. *The Lands of Partitioned Poland, 1795–1918*. (A History of East Central Europe, number 7.) Seattle: University of Washington Press. 1974. Pp. xvii, 431. Cloth \$14.95, paper \$7.95.

Students of East Central Europe have long lacked a basic, easily available, comprehensive history of the Polish lands. Neither Reddaway's *Cambridge History of Poland* nor Kieniewicz's *History of Poland*,

the former dated and the latter practically unavailable, has met this need. This volume successfully fills part of that gap. Wisely, Piotr S. Wandycz has avoided treating the nineteenth century as a history of the Polish question in international affairs. That topic is more than adequately covered in standard diplomatic histories. Instead, he focuses on the internal developments of the Polish lands, placing these within the context of European history, and traces the evolution of the national movement which eventually succeeded in re-creating an independent Polish state.

In dealing with the complexities of Poland—partitioned as it was among three empires, each with distinctive political, social, and economic systems—Wandycz uses a chronological approach. He follows the basic scheme of Polish historiography, dividing the era into four segments: the period from the Partitions to 1830; the revolutionary period, 1830–64; the reaction against revolution, i.e., the age of Organic Works, 1864–90; the road to independence, 1890–1918. Within this framework, individual chapters treat major trends and are further subdivided to cover events within the various components of the Polish lands, e.g., the Congress Kingdom, Galicia, Posen. In this manner Wandycz manages to make a complex story comprehensible. But this approach is unavoidably encyclopedic. For the most part then, the volume should be viewed as a handbook to the history of nineteenth-century Poland.

Among the book's many strong points is the evenhanded presentation of major events. No one subject, e.g. the November or January insurrections, is overemphasized. The stories of the minorities of Poland—the Lithuanians, Belorussians, and Ukrainians—which Polish historians normally ignore, receive fair and adequate treatment. Wandycz avoids polemics, skillfully weaves together the main currents of political, social, and economic developments, and takes the time to note, where relevant, major areas of disagreement in the interpretation of events. The author succeeds in *not* satisfying the reader; he whets one's appetite to learn more.

The encyclopedic approach does create some problems, however. Cultural developments are relegated to short chapters at the end of each major section. Perhaps this was unavoidable; but this treatment reduces cultural history to a compendium of names of famous Poles, known already to the cognoscenti but of little or no use to those unfamiliar with Polish history. A better treatment may have been to survey cultural history as a whole in one chapter.

In any work of this size minor typographical errors appear. For the most part these should cause the reader no problem. More glaring, how-

ever, is an error which can only be attributed to editorial carelessness. The leading political figure of the first half of the nineteenth century was Prince Adam Jerzy Czartoryski. Inexplicably, the first textual reference to him is as Jerzy Adam, an error repeated in the index.

Wandycz's bibliographical essay is an excellent survey of the literature on nineteenth-century Poland. Sadly, it also emphasizes a major problem for anyone interested in this era. The vast majority of citations are in Polish, highlighting the dearth of material in English. Those who lack a knowledge of Polish will, therefore, remain handicapped until monographs are written and published to illuminate Wandycz's work. In the meantime, this will remain the standard text for all European historians. Wandycz views his work as "both more and less than a history of the Polish nation in the nineteenth century." In the best sense of this concept the book, for the most part, succeeds.

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JAN KOSIM. *Okupacja pruska i konspiracje rewolucyjne w Warszawie, 1796–1806* [The Prussian Occupation and Revolutionary Conspiracies in Warsaw, 1796–1806]. (Polska Akademia Nauk, Instytut Historii, Pracownia Dziejów Warszawy.) Summaries in French and German. Wrocław: Ossolineum, 1976. Pp. 272. 70 Zł.

Since Warsaw was under Russian rule throughout most of the nineteenth century (actually from 1813 to 1915), it is sometimes forgotten that in the Third Partition of Poland the Polish capital was assigned to the Kingdom of Prussia. Much less has hitherto been known about the decade of Prussian rule than about the later French (Duchy of Warsaw, 1807–13) and Russian periods in the history of the Polish capital. This void has now been filled by the monograph under review.

As the title clearly indicates, Jan Kosim's book deals with two specific subjects: the Prussian occupation of the city and the revolutionary conspiracies within it. It is the author's thorough treatment of the first subject that makes for the significance and value of the monograph. The book is based largely on unexploited manuscript sources located in the Central State Archives in Merseburg, East Germany and in the Central Archives of Old Acts in Warsaw. Other primary sources, such as the contemporary press of Berlin and Warsaw, supplied data to make the description of the Prussian occupation more complete.

Some seven thousand Prussian troops made a ceremonial entry into Warsaw on January 9, 1796. The plans and objective of the occupation had

been worked out in detail during the preceding weeks. The Prussian garrison would have to be a very strong one, for Warsaw was regarded as "a center of Jacobinism." Prussian officers assigned to the garrison were those who had experience in dealing with the Poles in those territories gained by Prussia in the first two partitions and who fought the followers of Kosciuszko in the 1794 insurrection. The goal of the occupation was to transform the newly acquired territories into a province of the Kingdom of Prussia. "*Man müsse die Polen entnationalisieren*" was the opinion loudly expressed at the court of Frederick William II. Denationalization would be necessary in order to prevent the province from becoming "a state within a state" and its inhabitants from regarding themselves "as a separate nation" ("*als eine andere Nation*"). Kosim spells out in detail the procedure employed by the Prussian occupation authorities for achieving this goal.

To resist the process of assimilation and to keep alive Polish national spirit with its hope for regaining independence, Poles formed conspiratorial societies with central headquarters in Warsaw and affiliates in other cities. Both the Central Council (*Zgromadzenie Centralne*) and the Association of Polish Republicans, however, were small and short-lived organizations. Even so, they established and maintained contact with Polish exiles in Paris, with Polish Legions in Italy, and with Polish revolutionaries in the other sectors of their partitioned homeland. Most important, they perpetuated the tradition of the Kosciuszko Insurrection.

Kosim describes the Prussian methods of dealing with the revolutionaries. The garrison was enlarged and sentinels stationed throughout the city. A *Geheime Staats Polizei* was established. Persons suspected of being potential revolutionaries were moved out of Warsaw. Travel in and out of the capital and abroad was greatly restricted and closely controlled. Strict censorship, not only of the press, but also of private correspondence, was imposed. Elements of the Warsaw population regarded as dangerous were drafted into the Prussian Army and stationed in other occupied regions. Curiously enough, this last practice boomeranged, for with the appearance of Napoleonic armies on Polish soil in the fall of 1806, Polish soldiers in the Prussian army quickly deserted to the enemy. And the Prussian garrison in Warsaw fled in chaos even before the French reached the city, thus bringing the Prussian occupation to an end in November 1806.

CHARLES MORLEY
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LECH TRZECIAKOWSKI, editor. *Niemcy w Poznańskim: Wobec polityki germanizacyjnej, 1815-1920* [The Ger-

mans in Poznań: In Relation to the Policy of Germanization, 1815-1920]. (Studium Niemcoznawcze Instytutu Zachodniego, number 29.) Poznań: Instytut Zachodni. 1976. Pp. 472. 90 Zł.

Polish works on Poznań and other German-Polish borderlands during the partition period have traditionally dealt with two factors: the Prussian/German government and the nationally-conscious Polish people. This work acknowledges a third element, the German population of this region, constituting a significant minority of over thirty-five percent in the province of Poznań. In this sense, this work may represent an important new departure, and one approaches it with high expectations.

As it turns out, while one is not entirely disappointed, *Germans in Poznań* is not the comprehensive general examination which the title suggests. For one thing, it is one-dimensional (and still somewhat ethnocentric) in that it deals with Poznańian Germans mainly "in the face of the policy of Germanization," concentrating on their thoughts and actions with respect to Polish national aspirations. For another, even this subject is dealt with unevenly. Behind the broad title there exist in fact several specialized studies strung loosely together by a thin, conventional narrative. The first of these monographs-within-a-survey is Jerzy Kozłowski's exhaustive study of Poznańian-German politics during the 1848-49 period, making good use of older local history journals and reinforcing the conventional wisdom that these years saw the decisive shift from cosmopolitanism to nationalism among German liberals. Secondly, there is an extensive study by Aleksander Kramski of Poznańian-German political activities in 1903-11, apparently incorporating an earlier study available only in typewritten form. Kramski also contributes a detailed study of the response of Poznańian Germans to defeat and uncertainty from November 1918 to the beginning of Polish administration of most of this province in early 1920.

The remainder of the text, by these two authors and Bolesław Grześ, is more or less filler, borrowing heavily from relatively few standard works, such as the important contributions of editor Trzeciakowski to Prussian-Polish history. There are signs of hasty assembly, including clipped paragraphs out of context, the complete omission of treatment of the 1912-18 period, and the absence of a map to deal with a heavy dose of local place-names. The contributions of the three authors do not form a very harmonious whole. There are numerous repetitions, e.g., very similar analyses by Grześ and Kramski of the membership of the Eastern Marches Society (*Ostmarkenverein*), and three separate analyses of much the same traditional body of literature on provincial demography.

The result is a mixture of long passages on German history which border on common knowledge and several monographs containing much material of interest to the specialist. The authors adhere presumably to the Marxist-Leninist state church, but this is not a major factor aside from occasional bows to socialist dogma and the use of agitprop terminology (e.g., all the "patriots" are Polish, all the "chauvinists" are German). The authors are primarily striving to inform and can be faulted, if anything, for sticking too closely to their sources, often presenting half-digested annotated notes while shying away from original reflections upon underlying causes or extraprovincial connections. Kramski is perhaps an exception when he acknowledges the significant segments of Poznań-German society (i.e., aside from the SPD) opposed to the government's anti-Polish policies, and also when he deals frankly with the fact that SPD/USPD leaders in 1918-19 were just as hostile to Polish designs upon Poznań as were other Germans. It is clear here that nationality and not class was the overriding category in the German-Polish borderlands.

In the end, despite its informational value, this work remains interesting primarily because of its emphasis on the German element in the history of part of today's Poland. Perhaps this focus will spread to Polish historians of Silesia, Pomerania, and East Prussia, whose history from the Middle Ages to 1945 is represented almost exclusively by this element. For Poles to come to terms with this fact in the way that the present volume does would be a sure sign that they feel secure in their possession of these provinces.

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I. S. IAZHBOROVSKAIA and N. I. BUKHARIN. *U istokov pol'skogo sotsialisticheskogo dvizheniia* [At the Sources of the Polish Socialist Movement]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 411. 1 r. 53 k.

This latest work by I. S. Iazhborovskaia (and N. I. Bukharin) is an altogether successful attempt to document and explore the origins of Polish socialism in the years 1875-82. Iazhborovskaia is the most prolific contemporary Soviet historian of the pre-1917 Polish Left, and her expertise with the complex and interrelated national and social issues of the years 1895-1917 is apparent in the book's careful reconstruction of the diverse Polish socialist programs of the late 1870s. To be sure, the authors insist on the "lawfulness" of the processes at work during this period—the transition from the "petit bourgeois" to the "proletarian" stage of socialist ideology—and they maintain that the emergence of the first Polish Marxist party, the

"Proletariat" (1882-86), was the "natural" outgrowth of this transition. Still, their treatment of such widely attacked socialist-patriots as Bolesław Limanowski and Zygmunt Balicki is evenhanded and remarkably free of ideological intrusions.

The first section of the book, the background of Polish socialism, is dedicated primarily to refuting the thesis of the London historians Lidia and Adam Ciołkosz that the distinctions between "utopian" and "scientific" socialism do not apply to the history of the Polish Left. Iazhborovskaia and Bukharin skillfully support their counter-argument with a discussion of Polish participation in the squabbles of the First International and with an analysis of the materialist background of Polish Marxist thinking.

The second and most important section of the book, "The First Polish Socialist Organizations," details the histories of dozens of Polish circles from the Warsaw "Social-Revolutionary Society of Poles" (1875) to the Paris "Society for the Mutual Help of Workers" (1882). The book concludes with the creation of the powerful Congress Kingdom party, the "Proletariat." The sources for this section range from superb to barely adequate. The subsections on the Poznań movement suffer from a lack of archival research while those on the "Równość-Przedświt" group in Switzerland (1879-82) and on the Russian Gmina movement (1879-81) are reformulations of well-known material from published monographs and document collections by L. Baumgarten, T. G. Snytko, J. W. Borejsza, and A. Molska. The most original subsections of the book deal with the Galician movement. In the archives of L'vov (TsGIA USSR) and of Kraków (AP) the authors have located new materials which demonstrate that, despite a backward economy and a tiny working class, Galicia produced a vibrant and sophisticated socialist movement among Polish skilled artisans and intellectuals.

The most disturbing aspect of the book is its poor organization. In part, this is attributable to the subject matter itself, spanning Polish socialist circles from Vienna to Warsaw, St. Petersburg, and Paris. In part, too, the uninterrupted progression which the authors attempt to trace between the histories of these groups is not totally justified and confuses the reader more than is necessary. The encyclopedic quality of the book is both its main strength and weakness. There is no more complete monograph on these years in Polish socialism in any language, and none more difficult to follow.

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M. K. DZIEWANOWSKI. *Poland in the Twentieth Century*. New York: Columbia University Press. 1977. Pp. xiii, 309. \$14.95.

To write a national history covering a prolonged period of time always presents a challenge to the historian, for history is replete with achievements and failures, glories and faults. M. K. Dziewanowski, an experienced historian of his native land, is well aware of this. Having previously written two monographs, *Pilsudski: A European Federalist 1918-1922* and *The Communist Party of Poland*, along with numerous articles, Dziewanowski uses well his proven skill and accumulated knowledge in this outline of Poland's modern history. It enriches the already existing literature in English on this East European nation.

Because the present is a continuation of the past, *Poland in the Twentieth Century* begins with "Roots of the Present" (pp. 1-62). The development of the nation is followed by an analytical exposé of its internal as well as external problems during the Piast and Jagiellonian Dynasties and the partition period, up to the twentieth century, which found Poles "better organized, tougher, and more politically mature than their lighthearted, pleasure-loving ancestors who were responsible for the ruin of the Polish-Lithuanian commonwealth" (p. 62).

The rebirth of Poland and her experiment in independence, covered in only fifty pages, provided a breathing spell, a chance to build up national confidence and educate a new generation in the national spirit. The Nazi-Soviet aggression of September 1939 imposed upon the Poles extreme trials and destruction; six million Polish citizens, half of them Jews, perished during the German occupation. For every 1,000 citizens of prewar Poland, 220 perished. Yet, despite all the sacrifices and heavy losses, the end of the war brought new disappointment and new tyranny, this one delivered from the East by Stalin.

Territorial revisions, economic recovery, and adjustment to the new and strange Communist regime are discussed in chapters six through eight. To live in the shadow of the Soviet Russian empire is not an easy task for a proud nation rooted in Western Christianity and culture. But it is also not easy for Moscow and the totalitarian Communist system to control and rule such a nation. Contemporary Poland, indeed, "is a set of puzzling paradoxes and a maze of incongruities" (p. 253).

Poland's history can be seen in three different dimensions: triumph, suffering, and expansion to the East. The author's concentration is on the first and second. While the German and Russian menace to Poland is extensively exposed and discussed, Polish oppression in Belorussia and the Ukraine in the seventeenth and eighteenth centuries and Poland's ill treatment of her national minorities in the interwar period are only scantily treated. Dziewanowski, an outspoken federalist, is, however, aware of wrongdoing. He does not hesitate to

express criticism, and even Pilsudski's policy has found a critic in him.

Overall, this book is written in a lively style and language and is aimed at the student and general reader. It offers a balanced treatment of politics, the economy, culture, education, and social issues. Dziewanowski remains unimpressed with "historical romanticism," and he shows a preference for empirical interpretation, being aware of the limits of personal intrusion into history.

Several related illustrations, an index, and quite extensive notes and bibliography enhance the quality of the book. Also, the publisher should be given credit, for there are only a few typographical errors, despite the use of many foreign words and names throughout the text.

I have enjoyed reading this book and urge librarians and instructors to bring it to the student's attention.

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JÓZEF KOWALCZYK. *Za kulisami wydarzeń politycznych z lat 1936-1938, w świetle raportów posła Czechosłowacji w Warszawie i innych archiwaliów* [Behind the Scenes of Political Events from the Years 1936-1938, in Light of the Reports of the Czechoslovak Ambassador in Warsaw and Other Archival Materials]. Warsaw: Książka i Wiedza. 1976. Pp. 146. 16 Zł.

Polish interwar policies and politics provide a fascinating subject, and in the last twenty years a number of Polish historians have produced important and valuable monographs on these topics. This, however, is not one of them. The book is generally based on reports of Jurej Slávik, the Czechoslovak envoy in Warsaw, who in his reports to Prague often wrote about Polish opposition circles. But Józef Kowalczyk selects only a few of these reports dealing with minor political events during 1936-38, reports which he presents in three uneven chapters, thematically different and loosely connected with each other. The first chapter of twenty-five pages deals with assorted reports about General Sikorski's evaluation of the political situation in Poland and Europe. The second chapter of fifteen pages refers to some policies of the Peasant Party. In the third chapter of fifty-three pages Kowalczyk tries to resolve the question of whether Prague was a center of Comintern activities in 1930. He points out here that at the time the Czech police investigations could not uncover such a center in Czechoslovakia, but he makes no attempt to corroborate this with evidence from other sources. The rest of the book is composed of four annexes reprinting Slávik's reports about a Polish peasant demonstration at Nowosielce in June of

1936, his conversations with General Sikorski and Maciej Rataj, and the meeting of Sikorski with French Ambassador Noël.

It is difficult to understand why this book was published at all in its present form. Apparently it was written for the general public, but it will contribute little to their understanding of the period or the events discussed. The book lacks depth, historical perspective, and objective analysis; it does not explain anything of importance. It is not a reliable book. Students of the period would be advised to turn to Slávik's unpublished manuscript—"Moja pamět—živa kniha" [My Recollections—A Living Book], and especially volumes four and five entitled: "Moje poslanie vo Varšave—Rok 1936–1939" [My Mission in Warsaw, 1936–39]—all based on personal notes and copies of his reports.

In short, the general public, for which the book was written, will find here little which might expand their view of the period and of Polish political life during 1936–38, but the four reports printed in the supplement might be of some value to a historian who does not have access to the original material.

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HIERONIM KUBIAK and ANDRZEJ PILCH, editors. *Stan i potrzeby badań nad zbiorowościami polonijnymi* [The Present Situation of and Need for Studies on Polonia Communities]. Wrocław: Ossolineum. 1976. Pp. 671. 150 Zł.

This work, an edited transcript of an international conference on Poles abroad held at the Jagiellonian University in Cracow in 1975, suggests that now, when Poland's neighbors have recognized the postwar exodus and reshuffling of its population, Polish scholars are seeking to learn the effects of that diaspora. In particular they want to discover how overseas countrymen have retained or surrendered their traditional culture. Such interest in the matter is not new, but formal academic sanction at Poland's oldest university is. Thus the significance of this work is less what took place at the meeting and more the new high regard the Poles give to what should be called Polonia studies.

The intended scope of the sessions was vast; the structure of the publication is orderly and logical. The presentations of the nearly two hundred participants are divided generally into three sections: first, the methodological and theoretical; next, the regional and monographic; and finally, general summaries of discussions based on the aforementioned papers.

Most of the contributions are in the social sciences, with papers chiefly on demography, statistics, political science, geography, and sociology, a

distribution reflecting the academic bias of the meeting's organizer, Hieronim Kubiak of the Jagiellonian University. That emphasis exposed certain unintended ironies which pervaded the proceedings. While cognizant of the significant lacunae in Polonia studies, the sponsors and participants were uncertain how to remedy the situation.

For example, Kubiak, Zubrzycki, Rysiak, and others discern sensitively the new sociological definition of Polonia as symbol rather than a precise human collectivity. But, while asserting that even language may not distinguish an ethnic Pole, all papers in this volume are in Polish. Further, many called for new interdisciplinary approaches with sophisticated justification; yet, except for the suggestive essay on Polonia artists, the conference neglected the humanities, literature, folklore, and the arts. Finally, E. Stankiewicz, head of the National Library, and the historian M. Drozdowski conclusively demonstrated public responsibility at home and abroad for upgrading the still poorly organized Polonia materials. But it would have been more effective if the many sympathetic non-Polish academicians capable of acting on their suggestions had been at the conference.

The concluding section of the book, which surveys scholarship in the various regions is conveniently placed and revealing. Migration studies in Poland itself have grown rapidly in importance with the establishment of research centers over the last five years. Abroad the leading Polonia scholarship has been in Europe, particularly in France, and in Australia. Research on other continents is still in its infancy.

The North American Polonia is most crucial as it contains the bulk of the group abroad, but no overview of research in this area is evident. Gromada, Starczewska, Jost, Siemankowski, and others all gave important monographic papers, but no one attempted to synthesize current Polish American investigation. Perhaps that will come at a future congress as the area spokesman, W. Drzewieniecki, suggested.

That this volume spends so much space on deficiencies and problems ought not lead its audience to any overall pessimism. While the proceedings highlight the difficulties in mobilizing a new field of study worldwide, they also reveal the great intellectual rewards Poles and non-Poles will gain by joining the effort.

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MARCELI KOSMAN. *Drogi zaniku pogaństwa u Bałtów* [Roads to Extinction of Paganism among the

Balts]. (Polska Akademia Nauk, Instytut Historii.) Summary in French. Wrocław: Ossolineum. 1976. Pp. 287. 70 Zł.

Though the title suggests a comparative study of the disappearance of paganism among the various Baltic peoples, the author deals only with the Christianization of Lithuania from the Middle Ages to the nineteenth century. Marcell Kosman draws heavily on Soviet Lithuanian scholarship, but he has also done extensive research in Vilnius archives, long inaccessible to Poles as well as to Westerners.

Specialists in medieval and early modern Polish or Baltic history will value this book as a survey of Lithuanian church history. In 1386 the old cult leaders—the Lithuanian princes and boyars—accepted Christianity, but they did not propagate it vigorously among the villagers. In the absence of a pre-Christian priesthood that might have organized resistance to the new church (and stimulated missionary efforts and forcible conversions), the peasants continued to follow ancient religious practices undisturbed. Not until 1523 did Catholic synods begin to discuss the training of Lithuanian-speaking parish priests, and only in the early seventeenth century, when the Polish Catholic Reformation was well under way, were serious efforts begun to inculcate Christianity at the village level. Even then, three-quarters of the priests and virtually all Protestant ministers could preach only in Polish. Drawing on Catholic and Calvinist visitations, mostly unpublished, Kosman devotes most of his book to describing the presentation of Christianity through the creation of new parishes, attempts to improve clerical behavior, enforcement of compulsory church attendance, and publication of preaching manuals in Lithuanian. Priests encouraged saints' cults and established shrines at spots where wood spirits were worshipped. By the nineteenth century the rural population was—as Kosman puts it—at least externally Christianized.

The author raises more questions than he answers about the Lithuanian common people's assimilation of Christianity. He never explains the difficulties of both Catholics and Protestants in recruiting a Lithuanian-speaking clergy. Despite Königsberg's importance after 1543 as a center for the publication of religious literature in Lithuanian, Kosman's discussion of Ducal Prussian influence is minimal (pp. 192–95). He barely mentions (pp. 142, 231) the Jesuits' village missions, the rural parish brotherhoods, and the cult of St. Isidore the Plowman—all important in the Polish Catholic Reformation and significant also in seventeenth-century Lithuania in giving a Christian coloring to folk religious beliefs. The relative rarity of witchcraft incidents deserves more than the cur-

sory notice that Kosman bestows (pp. 226–27). Counter-Reformation preachers never hesitated to compare the nobleman's sins with the peasant's steadfast Catholicism, but Kosman would have the reader believe that only in the nineteenth century did the Church dare reduce its dependence on the *szlachta* and praise the peasantry (pp. 245–46). The author also is mistaken in stating (p. 23) that sixteenth-century visitors were unaware of rural paganism: Münster's *Cosmographia* (1544) gives much space to Lithuanian sun- and snake-worship, and both Catholic and Protestant reformers commented on this shocking manifestation of Satan's work.

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A. D. GORSKII. *Bor'ba krest'ian za zemliu na Rusi v XV-nachale XVI veka* [The Peasants' Struggle for Land in Russia from the Fifteenth through the Beginning of the Sixteenth Century]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1974. Pp. 212. 1 r. 13 k.

IU. A. TIKHONOV. *Pomeschchich'ie krest'iane v Rossii: Feodal'naia renta v XVII-nachale XVIII v.* [Landlords' Peasants in Russia: Feudal Rents in the Seventeenth and Beginning of the Eighteenth Centuries]. Moscow: Izdatel'stvo "Nauka." 1974. Pp. 334. 1 r. 68 k.

Both of these works are on topics of central concern to Soviet historiography, and each is a significant contribution. A. D. Gorskii, Doctor of Historical Science at Moscow State University, examines the peasant struggle for land in northeastern Rus' by applying a statistical approach (sampling theory, confidence intervals, correlation analysis, and others) to the 1607 relevant documents surviving from the years 1390 through 1506. The peasants are independent taxpaying peasants living on their own lands (which Gorskii considers to have been state land), court peasants living on royal lands, and seignorial peasants living on private lands engaged in disputes over land with usurping monasteries and some lay lords.

Gorskii found that the disputes were sometimes won by the peasants. To this he attributes some of the well-known Russian peasant "naive monarchism," the belief held by agriculturalists that the grand prince (after 1547, the tsar) was their protector—for in fact royal administration tried to be evenhanded upon occasion in adjudicating land disputes.

One wonders why there should have been increasing conflicts over the possession of land, which for most of the period was a "free good." Population was notably sparse on much of the

East European plain; title to land was often expressed by the formula "where the ax and plow have passed"; agriculture was often extensive—slash-burn, assartage, swidden; housing was nearly free; and moving was easy. (This provoked serfdom, binding the peasants to the land.) The dynamics of contention over land are obviously different in such a milieu than where land is scarce. Gorskii shows that contention over land increased, but he does not explain why. The answer probably lies in increased population at the end of the fifteenth century, which also saw the limited introduction of the three-field system of agriculture.

Iu. A. Tikhonov, Doctor of History in the Institute of History of the Academy of Sciences, examines the peasant situation after the land had been usurped by monasteries or mobilized by the government to support landlords who comprised the army in the Moscow region, between the Volga and Oka rivers, particularly Moscow, Galich, and Riazan' provinces. Relying on an examination of a massive quantity of primary sources (primarily over fifty thousand land allotment, confiscation, investigation, and census record books of the Service Land Chancellery, and especially those documents for about 365 estates for which rent data are extant), he periodizes his study into the era before the final enservment of the peasantry in 1649, the years between 1649 and the abolition of the landed-area system of taxation in favor of a household tax system in 1678–79 (which led to the creation of the extended family), and the years between 1680 and the institution of the male ("soul") tax system in the early 1720s. The lack of comparability of periods is immediately apparent, so the major break is really 1649. Attaining comparable units is also difficult when counting is done in terms of landed area, households, or males, and at times when prices fluctuated dramatically. In each section Tikhonov discusses the changing relations and proportions of the increasingly prominent labor rent, rent in cash, and rent in kind (the least significant) by region and type of landholder. The data are summarized in fifty-eight large tables.

Tikhonov's is the first general study of the entire region, beyond the holdings of a single landholder. One of his major problems is that lengthy series of sources covering the same places are very scarce, so the problem of comparability again arises. He unconvincingly holds that the Marxist position that serfdom usually arose out of labor rent is applicable to Russia at the end of the sixteenth century, but convincingly shows that binding the peasants to the land made possible a significant increase in rent. Rent did not increase much during the Petrine era because state taxes were rising rapidly.

Tikhonov makes no attempt to analyze the total "burden," or how it changed in the 150 years under review, because the peasant budget is unknown. While Tikhonov shows that serfdom enabled lords to raise their real income from rent, he is unable to demonstrate what impact market forces, particularly the development of the "pan-Russian market" in the seventeenth century, had on rent, to what extent the development of a market was responsible for the observed increase in labor rent. (Were lords striving to get marketable grain?)

Gorskii's book is most notable for its creative use of statistical techniques to analyze well-known documents. Tikhonov is to be commended for his industrious mobilization of vast numbers of unused sources and ingenious attempts to make reasonable comparisons. Both works show the strong side of current Soviet historiography.

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V. I. BUGANOV, editor. *Vosstanie v Moskve 1682 goda: Sbornik dokumentov* [The Moscow Insurrection of 1682: A Collection of Documents]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 346. 1 r. 61 k.

This collection of more than two hundred documents is a companion to V. I. Buganov's previous work, *Moskovskie vosstaniia kontsa XVII v.* (1969), and an addition to the sharp discussion between the author and N. I. Pavlenko (see *Istoriia SSSR*, no. 2, 1971; no. 3, 1973). The materials are edited to demonstrate that the events in 1682 were not simple reactionary uprisings of musketeers instigated by boyar opposition to Peter I, but were, rather, part of a broadly based, "antifeudal" movement. They show the violence beginning long before the major explosion in May and continuing well into January 1683, with a fearful and unstable government in tenuous control. Other groups were also involved along with the musketeers, as lesser military service personnel, townsmen, and slaves joined in violent protest, acting opportunistically on individual grievances. The slaves' actions are among the most interesting, detailed especially in the colorful and expressive threatening letter the slave A. S. Stepanov sent to his master (no. 5).

The entire collection is rich with information about subjects other than the uprising itself. Slavery, the size of Muscovy's musketeer forces, the character of the *Novomeshchanskaia sloboda* in Moscow, and the construction of fortress-towns are among the matters illuminated in these documents. Unfortunately, this work conforms to the Soviet practice of omitting a subject index, making indirect use of the collection more difficult than

necessary. There are, however, good indexes of persons and geographic places.

Most of the documents included are previously unpublished although a few of the major petitions are in AAE, PSZ, SGG i D, and DAI. Changes from prior published texts are carefully noted and generally minor, while the new texts are more complete. There are ample cross references in the interpretative commentary, excellent editorial notes, and a brief glossary of obscure terminology.

DAVID H. MILLER
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M. A. ALPATOV. *Russkaia istoricheskaia mysl' i Zapadnaia Evropa, XVII-pervaia chetvert' XVIII veka* [Russian Historical Thought and Western Europe, from the Seventeenth to the First Quarter of the Eighteenth Century]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 454. 2 r. 30 k.

Travel accounts and narratives of Muscovy and Petrine Russia are numerous and offer manifold riches. Noteworthy for their blunt observations, lively detail, and element of contemporaneity on the one hand, their subjective nature on the other alerts the reader to use them with caution. M. A. Alpatov has recognized the pitfalls but has exploited some thirty accounts to assess the development of Russian historical thought in its apprehension of Western Europe and to show Western perceptions of Russia and its history in the seventeenth and early eighteenth centuries. The study supplements the author's earlier one covering the same subject from the twelfth to seventeenth centuries. Alpatov contends that "historical science" emerges in Russia at that moment "when having freed itself from the providentialism of historical theory it is joined with a knowledge of historical facts" (p. 3). The implications of the terms "providentialism" and "historical facts" are not well clarified, but from later discussions we infer that Alpatov simply means the element of religiosity found in many of the writings, as well as the poor sources used in depicting Russian history.

The author has organized his study on a chronological and biographical framework. For the seventeenth century the works of Western authors only are discussed, which makes the title of the book misleading. Most of the foreign authors are shown as poorly prepared to interpret Russian history and as almost uniformly hostile toward the society they described. At the same time, they provided useful insights on many episodes of the Time of Troubles, the Razin Rebellion, and the Great Northern War that cannot be found elsewhere. It is the historiography of Petrine Russia, however, that Alpatov finds most significant. He carefully

illustrates Tsar Peter's encouragement of historical activities and provides excellent evaluations of the works of Kurakin, Prokopovich, Lyzlov, and Tatishchev. The latter is shown as the first true exponent of "historical science" in Russia.

In brief, Alpatov's study is well crafted and researched, and a book worth owning.

BICKFORD O'BRIEN
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G. A. NEKRASOV. *Rol' Rossii v evropeiskoi mezhdunarodnoi politike, 1725-1739 gg.* [Russia's Role in European International Politics, 1725-39]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 318. 1 r. 76 k.

Despite the title's promise of a broad treatment of Russia's role in European politics, Nekrasov's new book is in reality a continuation of his 1964 volume on Russo-Swedish relations after the Great Northern War. The author treats the major issues and alliances of the period, including Russian manipulation of the Holstein Duke Karl Friedrich's claims on Schleswig, the Hanoverian alliance that briefly brought England, France, and the Scandinavian powers together in a threat to Russia's newly-won position in the Baltic, the Vienna alliance, the Congress of Soissons, the War of Polish Succession, and the Russo-Turkish war of 1735-39. Yet all these questions are analyzed primarily with a view toward their impact on Russo-Swedish relations. The most obvious case is the Turkish war, which Nekrasov describes almost entirely through the eyes of the Russian minister in Stockholm. This approach reveals the author's heavy reliance on Russian materials relating to Sweden (collected in the central archives of Moscow, Leningrad, and Tartu), and while it helps him to make an important, if scarcely original, point about the interconnection of the Baltic and Black Sea questions, it is not conducive to a presentation of Russia's role in Europe more broadly.

By way of interpretation, Nekrasov makes the usual references to a dialectical understanding of international relations, all of which amounts to the stock Soviet justifications for Russian expansion (the need for economic development, "reunification" of Ukrainian and West Russian lands). In fact, about the only thesis the author proposes is European recognition of Russia's great power status, which he argues was achieved early in the century and finally accepted by other powers in the period treated here. Since Nekrasov reneges on his promises of sophisticated economic and internal political analysis, the principal value for Western scholars of this very traditional diplomatic history lies in its many documentary references and

citations from the rarely accessible Soviet Foreign Affairs Archive.

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RICHARD A. PIERCE, editor. *Documents on the History of the Russian-American Company*. Translated by MARINA RAMSAY. (Materials for the Study of Alaska History, number 7.) Kingston, Ontario: Limestone Press. 1976. Pp. viii, 220. \$9.50.

The 1957 Russian edition of this collection of documents, which are located in the Krasnoïarsk State Archives, was warmly welcomed by scholars—both Soviet and non-Soviet—interested in the Siberian fur trade and the colonization of Alaska. The extant primary sources are very limited and access to them is difficult. The documents in this collection, covering the years from 1793 to 1817, shed light on many aspects of Russian activity in the North Pacific that could only be seen in shadowy outlines, if at all, prior to their publication.

A few examples will indicate the value of this collection in helping to answer some of the questions that have plagued researchers in this area. Documents 20 and 21 give the amount of goods and products needed per person (employees of the company) per year while stationed in the Aleutians and Alaska and the cost in trade goods and transport of the furs obtained. This data will be valuable in ascertaining with a greater degree of certainty whether or not the fur trade was profitable for the company. Document 11, a description of the Tlingit Indians written in 1803, will be of interest to anthropologists as well as to historians. Additional evidence of the extreme dissatisfaction of the Russian employees with their treatment by the company is found in documents 5, 6, 7, 10, and 23.

Richard Pierce, editor of the series *Materials for the Study of Alaska History*, of which *Documents* is number 7, continues his useful practice of adding relevant maps and illustrations, as well as a glossary and index. The translation, with few exceptions, is accurate and reads well. Pierce is to be commended for making this excellent source available in English.

MARY E. WHEELER
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IURII IA. RYBAKOV. *Promyshlennaia statistika Rossii XIX v.: Istochnikovedcheskoe issledovanie* [Russian Industrial Statistics of the Nineteenth Century: A Source Investigation]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 276. 1 r. 32 k.

Iurii Ia. Rybakov's monograph purports to be a "comparative-historical analysis" of the basic statistical documents dealing with Russian industrial production in the nineteenth century. Arguing that such an analysis constitutes one of the central themes in Soviet historical science (p. 7), he further contends that such data cannot be utilized without first correlating it with the unpublished information available from government archives. At the same time, he also suggests that such efforts are fruitless without a sufficient use of Marxist-Leninist methodology (p. 270).

With regard to the former problem, Rybakov uses as an analytical tool the various compendia from the Department of Manufacture and Internal Trade of the Imperial Ministry of Finances as well as the miscellaneous data collections of selected factories, mills, and All-Russian industrial expositions. He draws on such collections as the Central State Historical Archive (TsGIA), the Central State Archive of Ancient Acts (TsGADA), and material from the Vladimir (GAVO) and Yaroslav (GAYaO) oblasts.

Throughout the book the author utilizes an impressive number of tables and charts to illustrate his points, covering such topics as the organization of collections, the nature of information contained in these records in the nineteenth century, and the process of publication of materials dealing with factory and mill production statistics during the period. He does not, however, directly begin to discuss the trustworthiness of such statistics themselves until the final quarter of the book and, although he claims that certain conclusions about the nature of Russian economic history in the nineteenth century can now be more fruitfully drawn, he makes neither his own methodology nor the nature of these conclusions very clear.

GEORGE E. SNOW
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B. N. KAZANTSEV. *Rabochie Moskvyy i Moskovskoi gubernii v seredine XIX veka (40–50-e gody)* [Workers of Moscow and Moscow Province in the Mid-Nineteenth Century (1840s–1850s)]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 181. 77 k.

Throughout the nineteenth century Moscow was Russia's single greatest manufacturing center. The Moscow region's distinctive fusion of peasant traditions and large-scale industry has been closely studied by such authors as M. I. Tugan-Baranovskii, G. von Schulze-Gavernitz, I. V. Meshalin, and M. K. Rozhkova. The present study provides ample evidence that the topic has not been exhausted. Untapped archival resources exist, and many questions remain unanswered.

B. N. Kazantsev's book consists of four substantive chapters, dealing with the overall pattern of industrialization, the formation and composition of Moscow's labor force, governmental regulation of workers and industry, and the material condition of the working class. The most original chapter is the one describing the labor force. Using archival material, such as the Moscow city passport registry, the author discusses such issues as migration patterns, work-force turnover, and family relations. Although his evidence is new, his conclusions repeat conventional wisdom about the workers' half-peasant, half-proletarian qualities.

The chapter on the government's mostly ineffective regulatory efforts is thorough and well documented, but recapitulates an article published by the author in 1963. The discussion of industrialization, on the other hand, is weak and narrowly focused. Kazantsev ignores the relevant conclusions of other authors (e.g. Tugan-Baranovskii, R. S. Livshits) and repeatedly cites secondary works for statistics whose original sources are readily available.

The neglect of other authors' work is evident elsewhere in the volume. Kazantsev asserts, for example, that state peasants were the greatest source of wage labor in Moscow; other scholars have suggested that manorial peasants outnumbered state peasants in Moscow's factories, but Kazantsev makes no mention of their studies.

Rarely does the author compare Moscow to other regions and time periods (much less to other nations), and as a result his discussion lacks perspective. In particular, Moscow's industrialization could have been contrasted to St. Petersburg's, especially in the field of cotton textiles.

In sum, although the work raises some new questions and presents a certain amount of new evidence, its focus is narrow and its conclusions and methodology are undistinguished.

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MIKHAIL BAKUNIN. *The Confession of Mikhail Bakunin*. Translated by ROBERT C. HOWES. Introduction by LAWRENCE D. ORTON. Ithaca, N.Y.: Cornell University Press. 1977. Pp. 200. \$12.50.

The recent revival of interest in Michael Bakunin's career and thought has centered around the large and impressive edition of his writings edited by Arthur Lehning. Other contributions continue to appear, including this translation—the first complete one in English—of Bakunin's controversial *Confession*. In his introductory essay, Lawrence Orton attempts to restate the issues raised in the

historiography over this unusual document. Bakunin had been arrested during the Dresden uprising in 1849 and was ultimately extradited to Russia, where he was placed in the notorious Peter and Paul Fortress in St. Petersburg. Two years later, with the encouragement of Count A. F. Orlov, head of the Third Section (national political police), Bakunin wrote a revealing account of his revolutionary activities. The manuscript was addressed to Tsar Nicholas I and was written in the explicit manner of a religious sinner expiating his wrongdoings to his spiritual confessor. The problem remains of determining what exactly Bakunin was confessing to, and why. If his real purpose was to convince the tsar to move him from the solitary confinement of the fortress (which he admitted he feared worse than death) to Siberian exile, he succeeded.

The *Confession* is rich in detail and analysis, particularly on Bakunin's perception of and involvement in the 1848–49 revolutions in Western Europe. On the other hand, it was carefully composed and is noteworthy for what it omits. Bakunin refused to give any hard information on the Russian or Polish émigrés in Europe, information in which Nicholas was most interested. His efforts at analyzing his own conversion to radicalism are opaque and far less significant than the discussions of similar matters in his letters to Herzen and Ogarev. It is difficult to believe that he was sincere in presenting himself as an errant heretic seeking absolution. At the same time, his own political commitment and the fervor of the barricades are sustained in spite of his effort to mute them (see pp. 55–56, 90–92, 110–12, 118–19). Psychologically, there is much of interest in Bakunin's narrative; he wanders erratically within the parameters of paternal ambivalence concerning the tsar, while also expressing both the mental fantasy and the physical reality of revolution.

The *Confession* raises more questions than it answers. It provides, however, an important piece of evidence that further illuminates the extraordinary complexity of Bakunin's personality. The translation by Robert C. Howes, which is taken from the 1935 Russian edition by Iurii Steklov, is very readable. Differences between this text and the first published edition of the *Confession* by Viacheslav Polonskii are noted in footnotes, as are the occasional banal comments of Nicholas I. Orton's end notes are quite comprehensive in identifying elements in the text which might be unfamiliar to the reader.

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ELIZABETH VALKENIER. *Russian Realist Art—The State and Society: The Peredvizhniki and Their Tradi-*

tion. Ann Arbor, Mich.: Ardis. 1977. Pp. xv, 251. \$7.50.

The *Peredvizhniki* have long been presented as next of kin to the political radicals of the nineteenth century and as ardent disciples of Belinskii and Chernyshevskii. Elizabeth Valkenier goes far toward demythologizing the realist painters by demonstrating how this image was formed, first through the work of Vladimir Stasov, then through the expurgation of contrary evidence during the Stalinist period. Her account of the systematic editing of the past to aid in the establishment of Socialist Realism should be of great service to all future researchers in nineteenth- and twentieth-century Russian art.

Valkenier presents the realist movement according to a classic pattern of birth, flowering, and decline. Her sections on the "embourgeoisement" of the *Peredvizhniki* and their adoption of a nationalist stance under Alexander III present these artists in a new perspective. While few could quarrel with her general outline, Valkenier makes some provocative assertions in discussing the motivation of the *Peredvizhniki*. She stresses their humble social origins and argues their inability to comprehend the political issues which excited their more informed contemporaries at the university. In her view it was a sense of inferiority, a need for "recognition" from the intelligentsia, which primarily dictated their actions.

Valkenier produces evidence for sins of omission and commission on the part of the *Peredvizhniki*. Their landscape paintings, for example, become suspect on account of their salability. In this case Valkenier adopts the attitude of the most doctrinaire of their critics, who saw landscape as lacking in "serious" content, although others valued its emotional appeal. *Russian Realist Art* is a much needed antidote to the Soviet canonization of the *Peredvizhniki*, although some readers may feel that the book tends to slight the positive evidence of social concern, which can be gleaned from the "heroic" period of the 1870s, and to make the personal failings of individual members grounds for condemnation of the movement as a whole.

Her final assessment of the realist painters rests on the fact that they were not revolutionary activists but chose most often to focus on the moral and sentimental dramas of the reform movement, in essence remaining close to the "denunciatory genre" of the 1860s. Valkenier rightly characterizes Kramskoi's attitude as one aiming for moral regeneration rather than political action. She also draws attention to Repin's portrayal of the revolutionary as an embattled and sometimes troubled figure rather than an idealized one—deducing from this a lack of genuine commitment to any

revolutionary cause. One might compare this view with that of Eugenia Herbert's *The Artist and Social Reform: France and Belgium 1885-98* (1961), which also notes—but without the pejorative tone—that the artists of the period responded more readily to the emotional or moral ethos of socialism than to specific programs of political action.

Russian Realist Art contains impressive research on the issues which engaged the realist painters. One hopes it will pave the way for further clarification of the origins and evolution of the *Peredvizhniki*.

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A. V. USHAKOV. *Revoliutsionnoe dvizhenie demokraticeskoi intelligentsii v Rossii, 1895-1904* [The Revolutionary Movement of the Democratic Intelligentsia in Russia, 1895-1904]. Moscow: Izdatel'stvo "Mysl'." 1976. Pp. 239. 95 k.

A. V. Ushakov's work treats the radicalization of segments of the intelligentsia that historians have neglected such as schoolteachers, *zemstvo* employees, and white-collar workers, who, by dint of their social origins, education, material situation, and life-style were closest to the people. His book is divided into five sections: "The struggle of teachers and medical workers"; "The opposition of the creative intelligentsia" (artists, writers, theatrical employees); "The strengthening of faith in oneself among salaried employees"; "The student movement"; and "Schoolboys in the struggle with tsarism." Defining these groups as the "democratic intelligentsia," Ushakov shows how their revolutionary movement occurred in two stages: the formation of mutual aid and self-help societies along occupational lines between 1895 and 1900; and the transition to political involvement in the struggle against autocracy between 1900 and 1904. This transition, he argues, was due to the revolutionary activities of the Social Democrats and the example of the increasingly militant workers' movement. Thus the "democratic intelligentsia" began to use "characteristically proletarian methods" such as strikes and demonstrations and to include "characteristically proletarian demands" such as the eight-hour day in their petitions for civil liberties and freedom of the person. Social Democrats, Ushakov stresses, realizing the usefulness of radicalized teachers, medical workers, and *zemstvo* employees (because of their closeness to the people), and recognizing the revolutionary potential of the "*chinovnik* proletariat" (Lenin's term), actively proselytized among them.

Ushakov's study provides valuable statistical in-

formation on the size, social origins, educational level, composition, activities, and material situation of the groups comprising the "democratic intelligentsia." He also indicates the heterogeneity of this wide-ranging category, which included rural schoolteachers, doctors, actors, and barely literate shopclerks. But he does not prove his contention that the transition of the "democratic intelligentsia" from occupational or professional activity to political activity can be attributed to the Social Democrats. They were indeed active, but so were liberals and Socialist Revolutionaries who are mentioned solely in negative terms. Ushakov notes, for example, that the 1904 banquet campaign of the liberals made them more visible, thereby creating difficulties for their Social Democratic opponents (p. 145). He also maintains that the "demagogic theory" and "adventuristic terror tactics" of the Socialist Revolutionaries attracted "unstable types" among the youth (p. 215), but details neither the influence and activities of these groups nor the struggle the Social Democrats waged against them. Thus a complete picture of the political physiognomy of the "democratic intelligentsia" does not emerge.

Furthermore, the ideological situation in 1904 (and indeed long after) was far more fluid than Ushakov indicates. A generalized revolutionary mood that affected all strata of Russian society facilitated the spread of Marxist ideas among the "democratic intelligentsia" but in combination (not necessarily logical) with other ideologies and attitudes including radical religion. These shortcomings aside, Ushakov's study provides a wealth of information on these under-studied groups and by implication sheds light on the increasing complexity of Russian society in the years 1895 to 1904 as the development of capitalism led to the formation of new social strata and to a new consciousness.

BERNICE GLATZER ROSENTHAL
Fordham University

BERNICE G. ROSENTHAL. *D. S. Merezhkovsky and the Silver Age: The Development of a Revolutionary Mentality*. The Hague: Martinus Nijhoff. 1975. Pp. 248.

In her commendable study, *D. S. Merezhkovsky and the Silver Age: The Development of a Revolutionary Mentality*, Bernice G. Rosenthal focuses on Merezhkovsky and his thought to enable her to develop the thesis that the esthetic trends of the Silver Age contributed to the evolution of the revolutionary mentality in Russia during the decades preceding the Revolution of 1917, thus ultimately furthering the cause of the Left, toward which Merezhkovsky himself exhibited a profound antipathy.

Following an introduction which offers a survey

of the social, political, and cultural events of the period 1890-1917, the work is divided into three sections (each beginning with a short general commentary), encompassing the years 1890-99, 1900-05, and 1905-17, conforming to what Rosenthal considers the three stages of Merezhkovsky's intellectual development: the "esthetic," the "religious search," the "theocratic." Although one might cavil at the arbitrariness of the divisions, they provide convenient frames in which to examine Merezhkovsky's contribution to the rise of Symbolism in Russia, his religious thought, and, finally, the period in which he applied this thought to the social sphere. An epilogue gives a brief outline of Merezhkovsky's activities as an émigré in France.

For the most part, the analysis and presentation of Merezhkovsky's ideas is apt. Yet, on occasion the desire to periodize while attempting to force a specific segment of Merezhkovsky's thought within a specific time-span is faulty. For example, to introduce the concept of the androgyne—a post-1920 development in Merezhkovsky's religious writings—into the period 1900-05 is misleading. There are a few factual errors: Merezhkovsky died in Paris, not Biarritz (p. 223); the novel *Leonardo da Vinci* is prefaced with the title *The Gods Resurrected*, not *The Birth of the Gods* (p. 101; the correct Russian title is, however, indicated in note 25 on the same page); the drama *Makov Tsvet* treats the Revolution of 1905, not the Russo-Japanese War (p. 147, n. 22); Merezhkovsky published only two novels, *The Birth of the Gods: Tutankhamon in Crete* and *The Messiah: Akhenaton King of Egypt*, after 1920, and not a trilogy of novels (p. 101 and p. 220). The Greek god Dionysos is invariably referred to as "Dionysios," a personal name deriving from the name of the god.

The above-mentioned points do not seriously detract from the study. While Merezhkovsky is the central figure, Rosenthal has been as concerned with the presentation of the period, as with the presentation of Merezhkovsky. There are very good, if necessarily brief, evaluations of the views of Merezhkovsky's fellow Symbolists, Briusov, Bal'mont, Bely, and Blok; of V. Ivanov and "mystical anarchism"; of Rozanov, Berdiaev, Bulgakov, and P. Struve, among others. There are comments on the impact of various journals—*Mir Iskusstva*, *Vesy*, Merezhkovsky's own *Novy Put'*, *Apollon*, *Zolotoe Runo*—and the meetings of the Religious-Philosophical Society on the development of contemporary values. As a result, *D. S. Merezhkovsky and the Silver Age* will be of interest and value to students of Russian intellectual and cultural history, especially as manifested in the fascinating three decades preceding the Revolution of 1917.

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E. E. KRUIZE. *Polozhenie rabocheho klassa Rossii v 1900–1914 gg.* [The Condition of the Working Class of Russia, 1900–14]. Leningrad: Izdatel'stov "Nauka." 1976. Pp. 298. 1 r. 53 k.

This book is a new entry in the debate about the nature of Russian society and its readiness for revolution in the years before the First World War. Its purpose is threefold: to prove the legitimacy of the October Revolution by demonstrating the significant size and maturity of the Russian working class, to describe the contemporary condition of the working class, and to supplant earlier surveys of labor conditions in this period, particularly A. G. Rashin's *Formirovanie rabocheho klassa Rossii* (1958).

The book presents little new information, however, even though it deals with a more limited time period than Rashin's; further, it cavalierly deals with the statistical sources employed, which include tsarist government statistics, Lenin, and Rashin himself. The author first estimates the change in the size of the Russian working class between 1900 and 1914. Where previous Soviet estimates find at most fifteen million workers on the eve of 1917, Kruize concludes there were 22.7 million Russians living primarily by wages in 1914 (although she concedes this was an increase of from only fourteen to fifteen percent of the population). To arrive at her figure, Kruize includes artisans, servants, and other non-factory workers whose role in the labor movement is too often unexplored by historians, but she also includes questionable categories such as workers of Russian origin employed abroad. (Since contemporary Soviets refer to "our" Chagall, why not also "our" workers on the Lower East Side?) To indicate the maturity of the proletariat, Kruize describes the social composition of the labor force by sex, origin, length of participation in the labor force, literacy. The discussion largely recapitulates Rashin; an exception is the attempt to measure the size of workers' families in order to determine the potential magnitude of the next generation, a qualitatively significant source of the increase in the labor force.

An overlong chapter on wages digests S. G. Strumilin's compilations for this period, cites differences by industry and by region, and concludes that nominal wages increased by some thirty percent from 1900 to 1914, but that real wages remained stable. There is inexplicably no new attempt to deal with the problem of the change in real wages, despite an extensive review of contemporary budget studies which might serve such a reconsideration. A final chapter on the duration of work presents the most new material, largely drawn from records of industrialists' associations, and suggests that hidden overtime more than com-

pensated for reductions in work hours won after 1905.

The book is substantively disappointing and stylistically repetitious and dull. Kruize's working class, despite geographic and occupational diversity, remains one-dimensional, a monolithic mass on the path to mobilization. This is all the more regrettable given the fascinating new directions suggested by the work of L. M. Ivanov and others. We can hope that the projected second volume of this work, on "Conditions of Labor and Daily Life," will turn more toward probing the complex texture of the Russian working class during this crucial period.

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JOHN L. H. KEEP. *The Russian Revolution: A Study in Mass Mobilization.* (Revolutions in the Modern World.) New York: W. W. Norton. 1977. Pp. xvii, 614. \$19.50.

Franz Borkenau once wrote of "the law of the two-fold development of revolutions: they begin as anarchistic movements against the bureaucratic state organization, which they inevitably destroy; they continue by setting in its place another, in most cases a stronger, bureaucratic organization, which suppresses all free mass movements." John Keep, well known for his history of the Russian Social Democratic Workers' Party during its formative years, has set himself the task of examining how Borkenau's law operated in the Russian Revolution of 1917. Unlike most previous historians, who have concentrated on political developments in Petrograd, Keep explores the social history of the revolution and discusses events in the provinces as well as in the capital. What concerns him, above all, is the experience of the lower strata of Russian society, workingmen, artisans, peasants, who were the actual makers of the revolution if not its ultimate beneficiaries. He focuses, in particular, on the grass-roots organizations, rural as well as urban, which sprang into life during the early weeks of the revolution and constituted perhaps its most striking feature. Here, says Keep, is where the pulse of the revolution beat most strongly.

The revolution, Keep shows, began as "an elemental popular movement inspired by the most egalitarian and libertarian ideals" (p. vii). Overwhelmingly decentralist and antiauthoritarian, it was a vast social upheaval, the greatest revolt of the century, in which ordinary men and women, in town and country, played the most essential part. For workers and peasants alike, the ideal society took the form of a direct democracy of councils, committees, and communes, in which they, the "toiling masses," could live in peace and con-

tentment with full economic and political liberty organized "from below." After the October seizure of power, however, these organs of popular self-rule—trade unions, factory committees, soviets, peasant committees, workers' militia—were taken over by the Bolsheviks and reduced, in accordance with Borkenau's law, to instruments of centralized authority, rubber-stamps of a new bureaucratic state.

With the aid of much recently published evidence, intelligently and convincingly deployed, Keep unfolds his story in rich and fascinating detail. Apart from his analysis of mass organizations, he provides a valuable survey, buttressed with a wealth of statistical data, of Russia's economic condition before and during the revolution. One of the great merits of his book is that it organizes a wide range of complicated material in an admirably clear and readable form. In doing so, it fills a conspicuous gap in the historiography of 1917. It is the most comprehensive treatment now available in English of the social dimension of the revolution, the work of an accomplished historian who can analyze and explain as well as gather and narrate. Our understanding of the revolution has been greatly enhanced by this book.

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A. IA. GRUNT. *Moskva, 1917-i: Revoliutsiia i kontrrevoliutsiia* [Moscow, 1917: Revolution and Counter-revolution]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 387. 2 r. 13 k.

Most Western scholars see the Russian Revolution as an event in one city, Petrograd, led by one man, Lenin. Yet the October Revolution in Russia's second city, Moscow, was more representative in some ways of the character of the whole movement. Like other cities in Russia, Moscow had its own local Bolshevik Party organization which had to make decisions for itself in 1917 without the benefit of direction from Lenin or other Bolshevik luminaries. Moscow also had a stronger middle class and an industrial work force that was more diverse economically and politically, and less radical. Also, the struggle for power in Moscow was more violent. There was a week of street fighting before the Bolsheviks achieved victory.

For all these reasons A. Ia. Grunt's book on the Moscow revolution is especially useful for Western scholars who wish to see the revolutions of 1917 from a broader national perspective. Grunt provides a comprehensive picture of the city of Moscow and its political life in the year 1917. He has been working on this subject for more than a decade, and he draws upon his own articles and all

other Soviet secondary works. The book's strongest feature is its abundance of factual detail, carefully sifted and weighed by the author. Grunt goes beyond the usual Soviet emphasis on party history to give the reader a detailed analysis of the city's social, economic, and political structure, with a special emphasis on important local organizations like the separate soldier's soviet and the district (*raion*) soviets.

In other ways the book is a disappointment. There are few striking new insights. It lacks the probing quality of some of the author's earlier articles. He tends to exaggerate civilian and garrison support for the Bolsheviks in the September elections to the district dumas and in the seizure of power. He tends to minimize the role of other political parties, of controversial figures like Trotsky and Bukharin, and of the idea of an all-socialist government. Most of all, he rarely uses archival evidence or non-Bolshevik newspapers, and does not even refer to relevant works by Western scholars, like Radkey, or émigré scholars, like Melgunov.

Like most Soviet scholars, Grunt is handicapped by the dictates of ideological orthodoxy. The groundswell of public support for the Bolsheviks in September 1917 was a complex movement throughout Russia in which the motives were different for each social group, army unit, and trade union. Yet Grunt must find every pro-Bolshevik vote as evidence of mass conversion to revolutionary consciousness, and as proof of Lenin's divine insight into the historical process. It is, therefore, not surprising that he skirts the question of mass participation in the seizure of power. The memoirs of participants make it abundantly clear that those who voted for the Bolsheviks were not prepared to go out into the streets and die for them during Moscow week, and that the struggle for power involved a relatively small number of people on both sides.

It is paradoxical, but one must turn to the doctoral dissertations of American scholars, like B'Ann Bowman Wright or Diane Koenker for a more balanced interpretation of the year 1917 in Moscow.

GEORGE D. JACKSON
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M. O. MALYSHEV. *Oborona Petrograda i izgnanie nemetskikh okkupantov s severozapada v 1918 godu* [The Defense of Petrograd and the Expulsion of the German Occupiers from the Northwest in 1918]. Leningrad: Izdatel'stvo Leningradskogo Universiteta. 1974. Pp. 110. 56 k.

The final operations on the eastern front in February and March 1918 brought the German army

within striking distance of Petrograd. The region west of the Russian capital—Latvia, Estonia, and Pskov province—fell under German occupation for nearly a year. This brief volume by a Soviet scholar examines the history of northwestern Russia (including Petrograd) from the Treaty of Brest-Litovsk to the successful Red Army offensive in this region during the final weeks of 1918. In the past two decades, numerous monographs and collections of documents have been published in the Soviet Union on specialized topics concerning Petrograd and the northwest in 1918: the formation of the Soviet Sixth and Seventh Armies, the extension of Communist Party control into rural regions, the Estonian and Russian war on German occupation authorities. Despite some delving into Soviet archives, Malyshev has made synthesizing this body of work his main goal.

Following an extensive and useful bibliographical survey, the core of the book consists of three loosely connected chapters. "The Strengthening of the Rear" focuses on the city of Petrograd: the spring food crisis, the hasty evacuation of heavy industry, and the Soviet regime's battle against subversion. "Under the Yoke of Occupation" describes the experience of Estonia and the Russian-inhabited regions west of Petrograd while under German control. Finally, "The Red Army in Defense of the Soviet Republic's Northwestern Borders" examines the construction of an effective military force in the Petrograd region, and its first successful employment against German forces and their White allies.

The book contains bits of information that will interest specialists. For example, Malyshev employs sources in Estonian to support his indignant view of German occupation policy. He has used the Central State Archives of the Soviet Army (TsGASA) in delineating the Seventh Army's order of battle and its preparations for the November 1918 offensive against Narva and Pskov.

Malyshev's useful passages nearly disappear, however, in a flood of evasion and misinformation. He finds Zinoviev, for no defined reason, opposing and obstructing all useful directives from Moscow. Trotsky has no role to play in the Red Army. Mensheviks, SRs, and their kulak henchmen mobilize rural opposition to the Soviet regime by "intimidation and trickery" (p. 31). Lenin is everywhere at once: the author of all wise policies, the scourge of evildoers, the judicious commentator on every major event. The unfortunate result is a tendentious account of heroes and villains.

NEIL M. HEYMAN

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V. A. VINOGRADOV *et al.*, editors. *Istoriia sotsialisticheskoi ekonomiki SSSR* [The History of the Socialist

Economy of the USSR]. In seven volumes. Volume 1, 1917-1920; volume 2, 1921-1925. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 448; 477. 2 r. 68 k.; 2 r. 82 k.

If readers no longer flinch at beginning a "History of the Soviet Union" with the Medes and the Persians, are we to object, except for petty logical reasons, to a history of the socialist economy of the USSR which begins before both the legal establishment of the Union of Soviet Socialist Republics (1924) or the proclaimed realization of socialism (1936)? These are the first two volumes of a projected seven-volume history of the Soviet economy. They have behind them the prestige of the Academy of Sciences and the first printing is 37,300 copies, an enormous number for such a press. The series is obviously intended to function as a standard reference work for a long time. The senior editor is I. A. Gladkov, who has been doing similar things for at least twenty years, who could, consequently, do it again in his sleep, and who has, it seems, done so.

The two initial volumes deal with periods very well covered in the literature, both Soviet and Western. They are, by the same token, particularly disappointing. They are textbooks whose references and citations, the soul of historical demonstration, are overwhelmingly taken either from other Soviet textbooks or from the works of Lenin and party resolutions, where every casual observation is treated as definitive proof of any proposition. While the collective authors are specialists on the topics assigned them, the treatments are not persuasive except to those conditioned to pontification. Marginally the most interesting contributor is V. N. Iakovtsevskii, who writes on agriculture and who raises some pertinent, if not altogether persuasive, theoretical points on the transformation of Russian agriculture.

Much space is devoted to denunciations of "bourgeois falsifiers" and, as is rarely the case in Soviet literature, some important scholars are attacked by name, notably E. H. Carr. Carr's volume on War Communism can indeed be faulted for its undue reliance on Kritsman, and Kritsman's book can and should be faulted for misplaced enthusiasm. Nonetheless, both Carr and Kritsman remain more interesting and much more informative than this dreary first volume.

As the preface of volume two acknowledges, it is based on *The Soviet National Economy in 1921-1925*, published in 1960 and edited by our same Gladkov. A curiosity, in the 1920s as now, is the role played by Felix Dzerzhinsky. Here he is presented as a hero of the Stalinist center. Elsewhere he has been shown as the protector of the sane ("bourgeois")

people (Valentinov) or as a John the Baptist of some Maoists (Grosskopf).

Hopefully, the later volumes will be more enlightening than the first two. Promised to come are "The Foundations of the Socialist Economy in the USSR (1926-1932)," "The Socialist Reconstruction of the Economy and the Victory of Socialism in the USSR (1932-1937)," "The Soviet Economy before and during the Great Patriotic War (1938-1945)," "The Restoration and Development of a Mature Socialist Economy (1946-late 1950s)," and "The Economy of Mature Socialism in the USSR" from the early 1960s to 1976.

As for the two first volumes, refutation of distortions, misrepresentations, outright lies, and so forth would take more space and time than I am allowed or would choose to use. They are less important as history than as contemporary documents, about as earthshaking as the new "Brezhnev" constitution.

DANIEL MULHOLLAND
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V. M. SELUNSKAIA *et al.* *Izmeneniia sotsial'noi struktury sovetskogo obshchestva, oktiabr' 1917-1920* [Changes in the Social Structure of Soviet Society, October 1917-1920]. Moscow: Izdatel'stvo "Mysl." 1976. Pp. 342. 1 r. 80 k.

Early in this volume, an effort at collective authorship by a number of Soviet social historians, V. M. Selunskaiia and V. S. Semenov discuss the "multifaceted" nature of social classes and the process of transforming class "structure." If "primary" social differences concern relations to production, they argue, a wide variety of additional factors determine class character: education, culture, conduct (*povedenie*), consciousness, social psychology, spiritual attitudes, political views, habits, and even conditions of everyday life (*byt*) (p. 6). Social "structure" is similarly multifaceted. It involves ways in which groups relate to each other, as well as their economic base. The transformation of classes and social structure is hence an alteration of subjective as well as objective conditions, exploitative attitudes as well as patterns of production and politics.

A promising start, all things considered. To my mind, a crucial question of Russian social history in the 1917-20 period is the degree to which social attitudes changed with social structure, and particularly, the extent to which those who secured controlling positions in Soviet society absorbed the attitudes and ethos of prerevolutionary class dominance, replicating in the process traditional (or "bourgeois") social patterns. Selunskaiia and her colleagues know the range of areas in which these

issues can be explored and appreciate their significance. But once the questions are posed, the authors back off, even from politically safe areas. Save for an interesting discussion of Western sociology (collectively authored by N. V. Naumov, B. I. Notkin, E. Ia. Vittenberg, and N. L. Rogalina), the volume slips into a routine presentation of major objective social changes. Naumov *et al.* have read Aron, Djilas, Bendix, Inkeles, Deutscher, and others, but they and the authors of subsequent chapters (V. Z. Drobizhev, A. M. Anfimov, Iu. A. Poliakov, Iu. S. Kukushkin, A. K. Sokolov and others) fail to explore the problems of power, attitudes, or even *byt* which Selunskaiia and Semenov lay out.

Perhaps the fact that most of the authors here are extremely able scholars unduly raised my expectations, and it is possible that volume's weaknesses are not primarily their fault. Still, the book is quite disappointing. There is far more assertion than proof, and much of the evidence itself is presented with an exactitude which undermines the editors' own strictures about inadequate data and faulty statistics. It is hard to deal with "facts" like the statement that 49.1% of the district soviet executive committee membership in 1920 was "worker," or that the "basic cadres of the working class" were "preserved" during the Civil War, despite mobilization, unemployment, migration, and famine. Sociologists might wish to peruse the early chapters for a serious discussion of methodology, and here and there some interesting summary material has been culled from published sources, largely available in the West. But no new archives have been mined, and no new arguments seriously presented. There is an index of authors. Lenin is cited on 153 pages.

WILLIAM G. ROSENBERG
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K. A. NAZIPOVA. *Natsionalizatsiia promyshlennosti v Tatarii (1917-1921)* [The Nationalization of Industry in Tataria, 1917-21]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 311. 1 r. 20 k.

The monograph under review pulls together the results of research on the nationalization of industry in Tataria which K. A. Nazipova and other Soviet historians have carried out since the late 1950s. It opens with an introduction which analyzes in cursory fashion Soviet historiography of the subject, describes the variety of sources used, lays out the themes and organization of the book, and sketches briefly the region's industrial development prior to the collapse of the tsarist regime in 1917. Three chapters follow, all of approximately

equal length, bearing the titles: "Workers' Control of Production and the Distribution of Goods as the First Practical Step toward Nationalizing Industry in Tataria"; "Carrying Out the Nationalization of Industry in Tataria"; and "Organizing the Administration of Nationalized Enterprises." The book closes with a short, rather perfunctory and ideological conclusion, an extensive bibliography (containing only Russian-language items, however), and a twenty-eight-page appendix listing regional enterprises and the dates of their nationalization. Unfortunately, as is still all too common for Soviet publications, there is no index.

Marxist-Leninist theory attaches great significance to the nationalization of industry as a major stage in the socialist revolution. This has prompted Soviet scholars to devote extraordinary attention to the subject and to produce in the process a vast literature tracing the drive toward full nationalization in the various regions of the USSR. Nazipova, however, is the first to write a comprehensive account for the region of Tataria (now the Tatar Autonomous Republic). During her years of research in party and state archives, she uncovered an enormous amount of information that permitted her to examine the subject fully. To the dismay of this reader, however, she seems to have crammed into her book nearly every bit of that information, no matter how trivial. And while innumerable facts and statistics flood the 275 pages of text, precious little in the way of synthesis or analysis—other than often crude applications of Marxist theory—accompanies the deluge. The study suffers as a consequence.

Nazipova's monograph will probably arouse very little interest in the West where work on Russian/Soviet local and regional history is still in its infancy. Nevertheless, those specializing in the economic history of the USSR, party history, or the history of the spread of Bolshevik authority beyond Petrograd and Moscow, may find this volume of value, if only for its use of archives and *fondy* inaccessible to Western scholars.

EDWARD J. LAZZERINI
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JANUSZ RADZIEJOWSKI. *Komunistyczna Partia Zachodniej Ukrainy, 1919–1929: Węzłowe problemy ideologiczne* [The Communist Party of the Western Ukraine, 1919–29: Crucial Ideological Problems]. Cracow: Wydawnictwo Literackie. 1976. Pp. 266. 35 Zł.

If this is not the definitive work on the Communist Party of the Western Ukraine (CPWU) in the 1920s, two factors are responsible: 1.) the topic is yet too controversial to speak of definitiveness, and 2.) even if Janusz Radziejowski did write the defin-

itive work, the Cracow publishers deleted much of the original text, especially portions concerning "internal Ukrainian themes" "going beyond the history of the Polish state at that time" (*Nasha kul'tura*, Warsaw, [1977] No. 1).

The end result is nonetheless a well researched and, within the limits set by the publishers, comprehensive treatment of the conjunction of Communism and nationalism among interwar Poland's Ukrainian minority. Radziejowski knows thoroughly the Soviet historiography on the CPWU (Halushko, Karpenko, Kravets'), but unlike Soviet historians, he does not shy away from Western and Ukrainian émigré scholarship (Solchanyk, Rosdolsky, Majstrenko). Nor does Radziejowski succumb to the vice of many Soviet historians of the party—projecting politics rigidly into the past and rewriting history accordingly.

Instead, Radziejowski displays independent judgment on a number of issues. He recalls Shakhrai's polemics with Lenin and points out that Lenin eventually came around to Shakhrai's point of view—that a loyal Communist could yet struggle for an independent Ukraine. He takes up Vasyliuk's argument that the Ukrainian nation had no indigenous bourgeoisie and shows that the theory was not so far-fetched. He revises the entire view of Kaganovich's role in the Ukraine, demonstrating, on the basis of archival sources, that Kaganovich in the twenties was not the anti-Ukrainian hatchet-man both Soviet and Western historiography have made of him.

The monograph particularly concentrates on the CPWU's complex relations with the Communist Party of Poland and the Communist Party (Bolshevik) of the Ukraine in the years 1924–28. Central problems are two splits in the CPWU over the national question. In Radziejowski's exegesis, the great importance of the national question in the CPWU derives from three circumstances: 1.) from the start, the CPWU saw its primary enemy not in the Ukrainian right wing, but in the Polish military and political authorities occupying Western Ukraine; 2.) the CPWU was an overwhelmingly peasant party and hence infected with the anti-Polish nationalism of the peasantry; and 3.) the Ukrainian leadership of the CPWU shared in the acute national sensitivity characteristic of the intelligentsia of a submerged nation.

The source base of the study is extensive. Radziejowski uses contemporary publications of the CPWU (now bibliographical rarities), interviews with former CPWU activists, and party archives in both Warsaw and Kiev. He quotes from and summarizes unpublished speeches and letters of such Soviet Communist leaders as Shumskii, Manuil'skii, Zatonskii, Skrypnyk, Kaganovich, and Zinoviev. Because of this, the monograph will interest

historians of the Communist Party of the Soviet Union as well as students of interwar Poland's political mosaic. For historians of the modern Ukraine, Radziejowski's monograph is indispensable.

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ROBERT C. TUCKER, ed. *Stalinism: Essays in Historical Interpretation*. New York: W. W. Norton. 1977. Pp. xx, 332. \$19.95.

Publication of a set of papers prepared for a scholarly conference inevitably involves the risk that they will lack cohesion or that they will be of uneven quality. As a counterweight, there is the hope that the papers will shed new light on important issues and will point the way toward needed research and fresh interpretations. In Soviet studies, books resulting from conferences, such as *Continuity and Change in Russian and Soviet Thought*, edited by Ernest J. Simmons (1955), and *The Transformation of Russian Society*, edited by Cyril E. Black (1960), have greatly influenced thought and scholarship in the field. This collection of essays, originally presented at a conference at Bellagio in the summer of 1975, will probably play a more modest role, both because the volume suffers from the difficulties of such an enterprise and because the topic is such a difficult and slippery one.

The book contains twelve interpretive articles by a range of East European and Western scholars, predominantly political scientists. It also presents one invited contribution from the dissident Soviet historian, Roy A. Medvedev, which is primarily scattered but interesting additional material to his *Let History Judge: The Origins and Consequences of Stalinism* (1971). Two of the articles deal with Stalinism in Eastern Europe, primarily Poland and Czechoslovakia; there is no treatment of the effect of Stalinism (or anti-Stalinism) in China or in Communist movements around the world.

The essays—and presumably the conference discussion—raise three basic and related questions. The first is a social scientific one: Is Stalinism a system that can be replicated, a model that can be discerned and analyzed in other times and places? Or is it a unique historical-cultural phenomenon, the product of a particular "cult of personality," or at least of the political will and actions of one immensely powerful and rather strange fellow? Clearly, no final answer is given here, nor perhaps can there be one. But analysis of the issue in several essays is well reasoned and challenging. Perhaps T. H. Rigby, an Australian specialist on the Soviet Communist Party, comes closest to creating a Stalinist model when he defines Stalinism as the combining of a mono-organizational

system with tyranny, but even he suggests in his last paragraph that Stalinism may have been a unique phenomenon.

A second major issue is the relationship of Stalinism to Leninism, Bolshevism, and Marxism. Two of the East European contributors, Leszek Kolakowski and Mihailo Marković, take diametrically opposed positions, the former maintaining that Stalinism was the natural progeny of Marxism, the latter insisting that it was a marked aberration from the Marxist world-view. Stephen F. Cohen argues forcefully that Stalinism represented a sharp break with Bolshevism, which he believes included Leninist, Bukharinist, and other tendencies. Again, no simple answer seems possible, and Robert C. Tucker, the conference organizer and chairman, appears near the mark when he concludes that Stalinism grew out of Leninism, that it emphasized, perhaps to the point of distortion, certain features of Leninism, but that it also rejected others, supplanting them with new and extreme measures and ideas.

A final theme is the impact of Stalinism on the Soviet Union and worldwide socialism generally. Here, where it might have been most helpful, the anthology is perhaps weakest. A number of important issues are alluded to but not explored. For example, one continues to puzzle over the acceptance of Stalinism—whom did it attract and why? Whom did it bedazzle, whom paralyze? Analogous questions relating to Nazism have been well analyzed, but in this volume only the essay by Moshe Lewin contributes to our understanding of this crucial aspect of Stalinism. Another case is the Russification of Soviet political culture, or the impact of nationalism on Soviet life, which Tucker notes as a significant characteristic of Stalinism but which is not examined in any of the essays. Moreover, as several of the authors admit, since the focus of the writings is almost entirely on the 1930s, insufficient attention is paid to the various phases of Stalinism, particularly its changed forms during World War II and in the postwar period. Lastly, no one directly addresses the key question of what lasting social, institutional, and psychological scars Stalinism has left on Soviet society. For current analysis and future understanding, that may be the most important legacy of Stalinism.

JOHN M. THOMPSON
American Universities Field Staff

NEAR EAST

PATRICIA CRONE and MICHAEL COOK. *Hagarism: The Making of the Islamic World*. New York: Cambridge University Press. 1977. Pp. ix, 268. \$18.50.

Hagarism: The Making of the Islamic World is truly a unique approach to the study of the transfusion of religious concepts to Islam from other varied religions of the Middle East and adjacent areas. In the first section of this 268-page book, replete with 118 pages of appendix, footnotes, bibliography, and general index, the authors explore in extensive detail contemporary non-Muslim documentation to make a case against Hagarism. Of course, the use of this word, which emanates from Abraham's second wife, immediately fastens the link in the chain which binds Islam to Judaism.

The authors note that, "This is a book written by infidels for infidels," and, as far as Muslims are concerned, "from any Muslim perspective must appear [to have] an inordinate regard for the testimony of infidel sources." It is further emphasized that much in the book will be disliked by the Muslim "who has lost his religious faith but retained his ancestral pride." The research is extensive, but I would question the use of certain terminology employed, which would confuse even the most accomplished Islamist.

From the very beginning Judaism and Christian Messianism did not serve the Hagarenes well, for they sought a positive identity of their own and theological originality. Muslims feared leaning on Christianity too much, since the result might have terminated in conversion. But a way was found; Arabs legitimized their practices of the Jāhiliyya, and the "Hagarenes contrived to make a religious virtue of the stigma of their pagan past."

Very little is accepted on faith by Patricia Crone and Michael Cook, but every phase of Islam is subjected to microscopic scrutiny. Here, after plodding through the work one is left with the feeling that there are no further stones to turn.

There is no question in the minds of Islamic scholars that much in Islam was borrowed from Judaism, Christianity, and other religions. Admittedly, Islam acquired its classical rabbinic form from Babylonian Judaism, and Islamic law was close to that of the rabbis. But on the other hand, little is acknowledged of transference the other way round. During the period of the exilarchate of Babylonia a strong symbiosis was established. Turning again to the Hagarenes further, their sanctuary was burned in a similar fashion to the Jews, they considered themselves chosen people—investing their lives with a religious aura. They took the priesthood of Aaron and "contrived the legitimation of an on-going authority." As far as literature and the legal system is concerned, "plots of Hellenistic dramas, the themes of Greek novels and thought and the scraps of Roman law provided materials for an Arab edifice."

In their discussion of the Koran the authors emphasize that it is lacking in overall structure, is

obscure, inconsequential in both language and content, perfunctory in its linking of disparate materials, imperfectly edited, and a hurried production. This sounds much like André du Ryer, who produced the early French version.

The research on Hagarism is thorough, but this reviewer feels that the conclusions drawn lack balance. The weights on the scales tip too easily toward the hypercritical side, tending to distract from what might have been an excellent study in comparative religion.

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HARRY AUSTRYN WOLFSON. *The Philosophy of the Kalam*. (Structure and Growth of Philosophic Systems from Plato to Spinoza, number 4.) Cambridge, Mass.: Harvard University Press. 1976. Pp. xxvi, 779.

This posthumous volume is the third work which the late Harry Wolfson, Nathan Littauer Professor of Hebrew Literature and Philosophy at Harvard, devoted to the problem of interpreting "Scripture in terms of philosophy" and of revising "philosophy in conformity with Scripture." In an earlier two-volume study of Philo, Wolfson dealt with the way in which this important Jewish philosopher had applied philosophical insight to the understanding of Scripture, and in another previous work he treated the Church fathers from a similar perspective. The present study of the Kalām, or Islamic scholastic theology, thus brings the study of this problem in the family of the revealed Scriptural religions full circle to its completion. All three studies are part of a long-term project to sketch "the structure and growth of philosophical systems from Plato to Spinoza."

The stated purpose of the work is "to describe the origin and structure and diversity of the teachings of the Kalam." It proceeds by following up every suggestion of possible foreign influence on the thought of the practitioners of the Kalām and then, by applying a method of conjecture and verification (a hypothetico-deductive method) to the relevant texts, seeks to establish the origin, structure, and diversity of certain problems of the Kalām. The book makes no attempt to survey the Kalām as a whole or to present the history of its development in systematic fashion. Rather, it confines itself to the discussion of those religious problems which are treated by the Kalām in philosophical terms; and among this limited range of problems it concentrates upon certain ones of the first importance, which are either exclusive to the Kalām or exhaustively treated by it. The book, therefore, contains a collection of material and an

analysis of the use Muslim thinkers made of philosophy in the resolution of the principal theological problems that faced the community. The specific problems that have been selected for treatment are the divine attributes, the Koran, Creation, atomism, causality, and predestination and free will. Wolfson shows how four of these problems are the same as those encountered in Philo and the Church fathers, and how the other two are outgrowths of discussions in the Church fathers. He also shows that each of these problems has an independent development in the Kalām and that each of them represents an attempt to explain Scripture in terms of philosophy.

Wolfson's book is an awesome monument of erudition, written by one who was a master of ancient and medieval philosophy, and a paragon of philological precision. It is far and away the most authoritative, penetrating, detailed, and altogether important book concerning the Kalām ever to have been published in English, and must be ranked among the half-dozen most significant works on the subject in any European language.

C. J. ADAMS
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STANFORD J. SHAW and EZEL KURAL SHAW. *History of the Ottoman Empire and Modern Turkey*. Volume 2, *Reform, Revolution, and Republic: The Rise of Modern Turkey, 1808-1975*. New York: Cambridge University Press. 1977. Pp. xxi, 518. Cloth \$29.95, paper \$13.95.

This second volume of the *History of the Ottoman Empire and Modern Turkey* by Stanford J. Shaw and Ezel K. Shaw is a major scholarly achievement that answers a deeply felt need for new perspectives and accurate, original information about the last century of Ottoman rule in South East Europe, the Middle East, and North Africa.

The value of this work stems from the use of massive amounts of original material found in the Turkish archives and supplemented with published works in various languages. The authors endeavor to present a continuous, systematic, and generic account of all the major internal developments in the Ottoman economy, administration, and society together with the biographies of chief political, military, and intellectual personalities who charted the course of Ottoman and Turkish history. Internal developments are carefully related to international events and Ottoman relations with the major European powers, including Russia. Finally, in contrast to scores of books on Ottoman history which deal with special events or problems, often exaggerating their importance and distorting the overall picture, this work strives to

provide a total view both of the Ottoman central political system and of events in its far-flung provinces.

The book essentially covers the reform age which got fully underway with the accession to the throne of Mahmud II (1808-39) and broadened gradually into a modernization movement in the Tanzimat period (1839-76) and especially during the reign of Abdulhamit II (1876-1909). Eventually it culminated in the Young Turk revolution (1908-18), and the establishment of a Turkish national state under a republican form of government (1918-75).

The book can be divided into two parts; the first, beginning in 1808 and ending with the loss of power by Abdulhamit II in 1908, and the second, covering a period from 1909 to 1975. The first part which is by far the best and comprises two thirds of the text, clearly shows that the changes initiated by the sultans aimed at creating a centralized bureaucracy and a new army capable of coping with the *ayans* (notables) in the interior and the foreign military threats. The *ayans* challenged the traditional government authority in order to consolidate their own socioeconomic position and possibly secure political power, while the European powers sought to gain strategic and economic advantages in Ottoman domains. The rulers' reformist endeavors, limited first to institutional changes, eventually created a momentum of their own, culminating during Abdulhamit II's time in a massive effort aimed at modernizing the empire's infrastructure, not in order to create a new sociopolitical system but with the purpose of establishing the material components necessary to assure the survival of traditional society and its sustaining foundation, the throne. Thus, during the latter part of Abdulhamit II's reign (1900-08) the railroad system reached 5883 kilometers and its revenue soared from 80 million to 740 million kurus, the highway system was lengthened from 6500 kilometers in 1858 to 23,675 kilometers in 1904, agricultural production doubled, the population increased, a modern press was established, and 1,358,508 pupils—out of a population of about six million people between five and twenty-five years of age—attended school. All this undermined the organization of the surviving classical Ottoman society and helped create, along with other social groups, a bureaucratic intelligentsia who yearned for power and for a vaguely defined liberal regime, and which eventually managed to topple the sultan from power.

The book provides a detailed and factual study of these internal dynamic developments and manages to shatter the image of the Ottoman state as the "sick man of Europe." It appears that ultimately the weakening and disintegration of the

empire was caused mainly by external forces rather than internal debility. A series of economic privileges granted to European powers led to the exploitation of the national economy, a staggering debt, which though stabilized during Abdulhamit II's reign continued to hinder investment, the non-economic philosophy of the ruling elite, the political and military pressures from abroad, and the perversion of the nationality problem into an ethnic and religious conflict, largely through Russian meddling, were some of the chief causes accounting for the Ottoman downfall. The failure of the Ottoman government to produce a new unifying principle of state capable of replacing the disintegrating traditional *millet* system and of superseding the claims of various ethnic groups and regional interests accelerated its own downfall, especially after the turn of the century. The Shaws studied the national movements among various ethnic groups, including the Armenians, using new statistical and documentary sources and found no evidence to back the stories of organized massacres or atrocities allegedly carried out by the Ottoman government in order to stem these national aspirations.

I believe that some of the problems faced by the Ottoman state in the nineteenth century, such as the low level of economic development, lack of industrialization, the nationality problem, and the social transformation, could have been analyzed better if placed within the conceptual framework of a colonial situation which was imposed gradually on the Ottoman society by the European powers, in alliance with merchants recruited from among non-Muslims, the upper echelons of the Ottoman bureaucracy, and other marginal elements. The failure to provide broad analytical concepts and insufficient recourse to some recent studies as well as a too-close attachment to some classical sources and models of analysis and a number of technical errors are some of the shortcomings of the first part of this otherwise valuable book.

The second section of the book consisting of three relatively short chapters, is devoted to the Young Turks, the defeat of the Ottoman government in World War I, the national uprising against Greek occupation and the establishment of the Republic. This section contains a sound factual account of events in 1908-75 but is less detailed and researched than the first part. Moreover, it lacks a central theme—reform was the linchpin in the previous part—to tie together the complex sociopolitical and ideological developments which produced a nationalist, populist, secular ideology that became the political watershed between the multiethnic Ottoman empire held together by the monarchy and a Turkish

republic based on national statehood. Nevertheless, the Shaws analyze some of these issues in the context of the six principles—republicanism, populism, secularism, nationalism, statism, and reformism—which formed the ideological basis of the early Turkish Republic until the Constitution of 1961 amended and made them compatible with the liberal pluralist order it legalized.

A book of this size and scope which breaks new ground and challenges many assumptions and stereotype images about the Ottoman Empire and the Turks is likely to provoke objections and criticism, particularly from those whose views were formed by some of the superficial and biased literature of the past on the Ottoman Empire. The book has an intrinsic value which will withstand such criticism. The extensive original material it uses, the new information it brings, the rich bibliography it contains, as well as the authors' balanced and critical approach make this book a milestone and a permanent indispensable source in the study of Ottoman history and society in particular and the Middle East and the Balkans in general.

KEMAL H. KARPAT
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J. BOWYER BELL. *Terror out of Zion: Irgun Zvai Leumi, LEHI, and the Palestine Underground, 1929-1949*. New York: St. Martin's Press. 1977. Pp. xi, 374. \$13.95.

The recent assumption of power in Israel by the former head of the *Irgun Zvai Leumi* terrorist group has given a sudden topicality to this account of the militant Zionist fringe of the 1940s. Menahem Begin would probably not dissent from the central argument of this book—that the *Irgun* and the even more extreme LEHI (or Stern gang) “were considerably more important than common wisdom would allow.” The author, who describes himself as “an innocent Episcopalian with slightly liberal leanings,” stresses that he has “no niche in the jungle of Zionist politics.” It must be said that, up to a point, his claim to disinterestedness is borne out in this book. There is much here which diverges from the standard *Irgun* version of Zionist history: in particular, the recent claim by Begin that the *Irgun* never intentionally attacked civilian targets is here exploded. The endless chronicle of bomb attacks, assassinations, and cynical vendettas, of which the victims were almost as often Jews as British or Arabs, is presented here in gory and sickening detail. J. Bowyer Bell writes in a breathless, boys' adventure story, “blood n' guts” style which may not endear him to an academic audience.

If the book succeeds to some extent as a chronicle, it fails dismally as an analysis. Like much of Revisionist Zionist historiography, this book is long on assertion and short on argument. Bell, in his preface, assails the "unsavoury and unproductive policies" of Weizmann and Ben-Gurion, but he produces nothing to show that the terrorists played a greater role in persuading the British to leave Palestine. He displays little understanding of, or even interest in, the internal dynamics of British decision-making in the Middle East. He too easily accepts the terrorists' own puffed-up images of themselves as fearless realists, and of the Jewish Agency and the mainstream *Haganah* as pusillanimous temporizers. On certain specific points of importance he appears to have been taken in by the terrorist propaganda version of history: he is, for example, too ready to accept that the Zionist leader, Arlosoroff, was murdered by an Arab rather than by a right-wing Zionist fanatic; and he swallows the martyrological tale that Stern (eponymous leader of the gang) was shot in cold blood by a British policeman. The latter is a story that was recently dismissed (with £4,000 damages paid for libel) in the British High Court.

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University of Sheffield

AFRICA

JOHN E. FLINT, editor. *The Cambridge History of Africa*. Volume 5, *From c. 1790 to c. 1870*. New York: Cambridge University Press. 1977. Pp. xv, 617. \$42.50.

Volume five, the second to appear of the projected eight-volume *Cambridge History of Africa*, provides a useful overview of the continent's history. It will serve as an indispensable source for comprehending and placing into perspective the divergent themes of historical process then occurring among the many peoples of Africa. Since historians of Africa endeavoring to develop frames of reference free from European-derived guideposts have sought a periodization of the continent's history growing out of indigenous themes, the editor of volume five, John Flint, presents a justification for the book's organization. The beginning and ending dates obviously are drawn from non-African events: ca. 1790 for the start of the British anti-slave-trade movement and occupation of South Africa, plus Napoleon's invasion of Egypt; ca. 1870 for the intensification of European involvements, ending in the near total conquest of Africa. "The period," says Flint, "can thus be considered as one dominated by the theme of Africa's growing con-

tact with Europe, as a time of slow penetration and preparation by Europeans for the coming partition and colonial rule." He admits that this choice of time perimeters, since most Africans lived their lives free of European contacts, appears "a somewhat irrelevant pattern for the history of the continent as a whole." While confessing his preferred desire for "a pan-African" approach, Flint concludes that at present the necessary corpus of sound scholarly spadework is lacking for the elaboration of broad African-inspired themes of historical evolution. Thus, in this volume, Africa was divided into cultural regions, each receiving separate treatment. Certain African-inspired themes did emerge from this approach, especially those relating to state formation and the effects of long-distance trade. This periodization of Africa's past is reasonable; certainly few satisfactory alternatives have found general acceptance. Nor is the dwelling upon African themes within a period defined by European guidelines surprising. The authors in the volume (twelve Europeans and North Americans, three West Africans) derived their historical training from universities operating in the historical traditions of Western Europe.

In volumes of this type all depends upon the individual authors selected for each chapter. Volume five profits from the labors of a competent group of scholars, chosen from a slightly broader intellectual environment than the London University-dominated cast of the previous volume of *The Cambridge History of Africa*. In a book dealing with an entire continent every reader inevitably will find regions and peoples receiving less consideration than he thinks proper. I found two instances particularly noticeable. In the too-brief chapter on the Maghrib, Douglas Johnson devotes almost his entire narrative to Algeria: Tunisia, Morocco, and Tripoli combined receive only one-third as much coverage. And, since this review was written during a residence in Mozambique, I was forced to notice its almost entire exclusion from the volume. In my judgment the most successful chapter is M. Hiskett's analysis of West African *jihads*. His carefully structured and reasoned account, based upon the internal happenings of the region, at the same time skillfully makes clear the impact of the broader Islamic world. As a result the lives and contributions of a host of important individuals—al-Maghili, Usman dan Fodio, al-Hajj Umar, to name only a few—are brought forward in masterly fashion.

The thought-provoking excellence of Hiskett's chapter led this reviewer to rethink his overall conception of volume five. What originally appeared as a reasonably satisfactory structure now emerged, because of the general treatment of Islam outside of Northern and Western Africa, as decide-

dly less so. Certainly the Muslim Arabs did not influence the majority of Africans. Nonetheless, their influence, of seminal importance in cultural and political matters, was worthy of the same careful analysis given to Christian European influences. But in this volume the Arabs and Islam often receive mixed treatment. In the chapters on the forest and savanna regions of Central Africa, by David Birmingham, and on East Africa, by A. C. Unomah and J. B. Webster, both capable pieces of scholarship, it is possible to take exception to the thinking concerning the Arabs. A footnote on page 244 informs the reader that "the term 'Arab' has often been applied to coastal Muslims more properly called Swahili." To remedy this supposed defect the term "Swahili-Arab" is used throughout the chapters. But not always. When Africans are exploited, the exploiters are "Arab plantation owners," (p. 298), not Swahili-Arabs. It is implied that the categories "Arab" and "African" are separate entities, the latter possessing their own "culture, race, and language" (p. 244). This is not a useful approach, making meaningless the wide diversity of an Arab world comprised of individuals belonging to many different ethnic backgrounds. It reminds me of the puzzled comment of British explorer Verney L. Cameron, made a century ago after meeting Tippu Tip, the most famous Arab of the East Central African interior. "And notwithstanding his being perfectly black, he was a thorough Arab, for curiously enough the admixture of negroid blood had not rendered him less of an Arab in his ideas and manners" (*Across Africa* [London, 1877], 2: 12). Tippu Tip considered himself an Arab; many of his contemporaries living in the Nile Valley, Algeria, and elsewhere thought the same of themselves. If scholars separated the cultural designation "Arab" from the geographical one of "Arabian" their thoughts on this matter might become clearer.

The lack of consideration of matters Islamic carries over into other sections. It is arguable whether Robin Hallett's chapter on European attitudes toward Africa should stand in a volume lacking the space to treat adequately many sections of Africa. If the answer is yes, then certainly a chapter considering the relation of the Islamic world to Africa similarly merits inclusion. Again, Christopher Fyfe presents a characteristically able account of freed slaves in West Africa, emphasizing the important rise of a group influenced by Western culture. Why not a similar treatment of Muslim-influenced Africans who played equally influential roles in African history?

Volume five is a worthy addition to the line of Cambridge histories. If the treatment of Islam throughout the book had matched that given to

African and European developments, it would have been a distinguished one.

NORMAN R. BENNETT
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A. J. CHRISTOPHER. *Southern Africa*. (Studies in Historical Geography.) Hamden, Conn.: Archon Books. 1976. Pp. 292. \$15.00.

The somewhat limited theme of this historical geography is the transformation of the landscape of Southern Africa from 1652-1960 through the agency of European settlement. The author shows that from 1815-1914 Southern Africa attracted only 4 percent of the emigrants from the United Kingdom compared to 11 percent who went to Australasia and 17 percent to Canada. The main reasons for this were the unfavorable assessment of the region's physical environment and the presence of a large indigenous population. As a result the European population grew slowly because there was no place for unskilled white workers until the mineral discoveries of the late nineteenth century.

In 1820 and 1850 nearly ten thousand settlers came from the United Kingdom and added substantially to the existing white population, largely of Dutch origin. The Great Trek of 1836 was another important event in the extension of white control over Southern Africa when about ten thousand white pastoralists trekked into Natal and later set up republics in the Orange Free State and Transvaal.

There is no mention of the fatal impact of disease on the Hottentot population, and little about the fate of the Bushman, or their artistic contribution to the life of Southern Africa. The Hottentots were either absorbed into the Cape colored population or, like the Bushmen, retreated beyond the frontiers of white settlement. Treatment of the Africans is inadequate, apart from the last chapter, and there is too much emphasis on the part played by Europeans, although the author does refer to current research on African oral history. There is no reference to the use of air photographs and only passing reference to archeological and protohistorical evidence that might help to reconstruct the landscape and events of the pre-European period.

The author traces the growth of European urban and rural settlement and makes good use of maps and diagrams in this connection. The quality of the illustrations, however, is poor. The spread of white control in Southern Africa is shown by the increase in the number of administrative units from 6 in 1806 to 252 in 1911. The effect of the discovery of diamonds and gold was to break down isolation with the rapid growth of economic nodes

in the interior. By 1896 Johannesburg had a population of sixty-one thousand, and by 1915 there were nearly eleven thousand miles of railway in Southern Africa.

The events of the period from 1911–60 are outlined, illustrating the concomitant rapid growth of European commercial agriculture and manufacturing industry, and the Southern Transvaal urban zone, which attracted large numbers of Africans, many of them migrant workers housed in mining compounds. Europeans and non-Europeans became separated residentially and a plural society developed. There is a brief account of the impact of European political theories on the landscape, but the wider significance of the political relationship between African and European and its effect on the contemporary scene receives scant treatment. The bibliography and individual chapter notes and references are useful.

N. C. POLLOCK
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Oxford University

B. G. MARTIN. *Muslim Brotherhoods in Nineteenth-Century Africa*. (African Studies Series, number 18.) London: Cambridge University Press. 1977. Pp. xiii, 267. \$19.95.

B. G. Martin has written an excellent comparative treatise on sufi brotherhoods in Africa. While many individual case studies have appeared on the leaders and groups he mentions in recent years, his study is one of the very few which attempts to show the differences among these groups in terms of their relationships to other Islamic brotherhoods elsewhere in the Middle East and Africa, their indigenous significance politically and socially, their reaction to colonial intrusion, the character of the various leaders involved, and the religious scholarship and philosophy of the latter. Furthermore, Martin uses available Arabic documents written by African leaders rather than relying solely on European and Middle Eastern sources, as some writers of the early case studies were forced to do. Thus he bases his work on the broadest available data, which he pulls together in a competent scholarly fashion.

The groups and leaders described are probably the best known in the history of sufi brotherhoods in Africa. They include Usman dan Fodio (1754–1817) in northern Nigeria, Amir 'Abd al-Qadir (b. 1807) in Algeria, Al-Hajj 'Umar Tal (1794–1864) in Guinea, Senegal, and Mali, Muhammad 'Ali al-Sanusi (1787–1859) in Libya and the Sahara, Ma' al-'Aynayn al-Qalqami (1831–1910) in Mauritania, Sayyid Muhammad 'Abdullah Hasan (1864–1920) called (although inaccurately) the Mad Mullah of

Somalia. There is also some discussion of the Qadiri and Shadhili brotherhoods in East Africa in the period 1880–1910.

Martin writes in a descriptive historical style, painstakingly recording known facts about ancestry, upbringing, travels, and accomplishments of the leaders, and the impact of their brotherhoods. He places his descriptions of all groups in the context of the developments in each area in the relevant period. Thus the reader has a framework into which the individual orders fit. However, the groups are so disparate and the situations so varied, that only the basic similarities, coming from the overall common period of turmoil and change in the precolonial and colonial era in Africa, and a few glaring distinctions, can be understood. The subtleties of the groups and their leaders are difficult to comprehend. It is ironic that a major criticism of Martin's book may be that he provides an overwhelming deluge of detail (such as Arabic names and genealogies and technical terms) while simultaneously he may be criticized for not providing enough depth on many of the orders he describes.

No one but a scholar already interested in Islam would find it easy to wade through the rather dry—but extremely accurate—descriptions provided. Yet such a scholar may be frustrated, after making the effort, by having learned no more than he would have by reading already published material on the subject. Such criticisms, however, must quickly be mitigated by an overall judgment that Martin has done the best possible job, given the sources available and the objective he set initially. He can not be blamed for lack of data which will continue to become available as more Arabic documents are located (and more accounts recorded). He has pulled together in a masterly fashion all existing sources. The careful comparative accounts he provides are very useful for those interested in the subject of African Islamic brotherhoods and the history of Africa generally.

LUCY E. CREEVEY
University of Pennsylvania

J. MUTERO CHIRENJE. *A History of Northern Botswana, 1850–1910*. Cranbury, N.J.: Fairleigh Dickinson University Press. 1977. Pp. 316. \$16.50.

This book is heavily charged with incident, personalities, and detail drawn from the missionary records and literature which form the main corpus of Botswana's historical sources. It should therefore stand as the major narrative of nineteenth-century Tswana social and economic history. This history is the story of how Tswana chiefs and people, dealing with missionaries and traders,

Boer interlopers and imperial administrators, achieved a dynamic balance between traditional and westernizing forces and how they established the basis of Botswana's successful emergence as a nation state in the baleful shadow of the Republic of South Africa.

Tswana chiefs actively sought missionaries and traders, and eventually welcomed the imperial protectorate, as agents of military security and diplomacy against each other and the encroaching Boers. They thereby also saved themselves from Rhodesia's fate by frustrating Rhodes' bid for Tswana territory. Dynamic tension characterized the connection. The Tswana expected to manipulate the Europeans for practical material advantage. Missionaries sought spiritual dominion and cultural revolution. They gained the formal conversion of major leaders such as Livingstone's friend Sechele and the great Kgama whose descendant is now the president of Botswana. Yet Tswana society did not cave in to the missionaries and proconsuls. By 1910 only 2,000 of the 90,000 Tswana were London Missionary Society communicants, although their influence was disproportionate to their numbers. The dynamic tension was exemplified in education. Missionaries proved reactionary as educators, interested only in catechism and obedience. Tswana youth and parents, however, demanded reform to supply vocational schooling that would enable them to cope with the failure of traditional economy which the European advance had precipitated. J. Mutero Chirenje concludes that this restored the traditional approach to education as in the initiation schools, a view that needs to be qualified by the difference in style and content between modern and traditional schooling.

Indeed, one feels that Chirenje's approach suggests a little too strongly that European expansion and dominance depended more on the volition of Tswana rulers than upon the dynamics of imperialism. He seems to overemphasize the degree to which chiefs or people controlled forces they had, perforce, to accommodate. In the end they had to react to external powers, but their greatness lay in judging and balancing the possibilities and necessities of their marginal situation in terms of power, geography, and resources. They eventually proved more creative than their ostensible mentors. They themselves forced the pace, pressing reactionary churchmen and political administrators to open colonial institutions to participation and change. There is a lack of reference to the increasing body of literature on the missionary role and cultural impact in Africa. At one point the author footnotes such studies as relate to Rhodesia, yet some reference to similar material on Ghana, Nigeria, and East Africa might have offered an opportunity to

relate Botswana's history to more general analyses of the missionary factor in Africa's colonial age.

MAYNARD W. SWANSON
Miami University

W. F. GUTTERIDGE. *Military Regimes in Africa*. (Studies in African History, number 11.) London: Methuen and Company; distributed by Barnes and Noble, New York. 1975. Pp. 195. Cloth \$12.75, paper \$7.00.

The first question to which a reviewer must address himself is that of the purpose of a book. Although W. F. Gutteridge spends some time discussing the causes of coups d'état and what is generally referred to as "the politics of coup d'état," he strongly suggests that this is not the main focus of his book. Clearly the author's main concern is to analyze the public policy performance of African military governments. Apparently, Gutteridge is interested in whether these military governments can succeed where civilian predecessors failed. It is one of the few disappointments of the book that the author does not fulfill his promise. Consider the following points: In discussing military rule in Ghana, he devotes fifteen pages to the politics of coup d'état, leaving only fourteen pages or so for a discussion of specific aspects of performance by Ghana's military governments; his discussion of the role of the military in Nigeria again concentrates on the politics of coup d'état, adding virtually no new knowledge to that which we have. It must be conceded that the last few pages, where the author outlines the performance of Nigerian military regimes are, on the whole, good, but it would have been more productive for him to elaborate upon and analyze these performance records.

The discussion of Mobutu's Zaire is quite interesting although recent event and reports clearly intimate that Mobutu's performance record may be overrated here. The assessment of the economic performance of the military regime should not have ignored the staggering and continuing inflation. In addition, the short chapter on Uganda is somewhat disproportional in this respect, though one can sympathize with the author because sometimes it is hard to distinguish between fact and fiction in Uganda. The chapter on the Sudan, despite its brevity, is one of the most interesting of the case studies. Here the author would seem to have fulfilled the goal which he set out for himself.

The discussion entitled "A Professional Legacy of Colonial Rule" sometimes betrays the sympathies of the author. For instance, it is bewildering that anyone could interpret Nkrumah's exhortation to "seek ye first the political kingdom" as evidence

that Nkrumah would "not in the first instance have considered settling for anything other than a Westminster style parliamentary system . . ." (p. 24). Nkrumah's political kingdom is independence, not the model of Westminster. Moreover, it seems fair to say that Nkrumah's acceptance of the Westminster model was a concession to pragmatic considerations, or, as he would put it, a tactical move. This interpretation is more consistent with his actions and, therefore, a more plausible interpretation. I do not mean to suggest that this chapter is counterproductive. Indeed, Gutteridge's discussions of "the professionals," the questions of "cultural and institutional transfer," "the curricula," and of African experience in British military officer-training schools are remarkably refreshing, interesting, and informative.

Gutteridge does not share the view that the extent to which a politicized officer corps plays a conservative or reforming role is a function of the expansion of political participation in society. He argues that this view is an oversimplification of "the process of political development." I wish he had elaborated further on this interesting point. He also feels that this view fails to distinguish between military institutions of different societies. In his judgment, it assumes that military officer corps are necessarily middle-class oriented.

Gutteridge also thinks it is misleading to isolate the military institution from the social structure of the society within which it operates. Quite frankly, this is the central weakness of the formal organization model, the mass deterministic perspective of Huntington and the reference group model of Prize. With regard to the latter, Gutteridge is forthright, precise, and perceptive: "Explanations of military political behavior which attribute almost the whole responsibility to the colonial power must be viewed with great caution, for armies themselves do not often actually create the social circumstances conducive to their intervention in politics" (p. 25).

In reference to "the scenarios for military intervention in Africa," the author asserts that the "effective presence at a given time of a single personality whose personal idiosyncrasy, skills, or unscrupulous opportunism, or possibly state of mental or physical health" has been underrated. True enough but deadly accurate, and most unfortunately so.

VICTOR A. OLORUNSOLA
Iowa State University

VICTOR A. OLORUNSOLA. *Soldiers and Power: The Development Performance of the Nigerian Military Regime*. (Hoover Institution Publication, number 168.) Stanford, Calif.: Hoover Institution Press. 1977. Pp. xiv, 168. \$8.95.

Victor Olorunsola sets out to measure the "development performance" of General Gowon's Nigerian government over the period 1971-74, both objectively from such statistics as are available, and subjectively as perceived by students, university staff, farmers, traders, journalists, workers, and the ruling military elite. Development is broadly defined to embrace not only economic matters (GNP, unemployment, inflation, etc.), but also social and political progress (literacy, more equitable income distribution, national integration, etc.). Fifteen indicators of development are listed in the first chapter.

It is not easy to measure all these parameters in the absence of reliable, or in many cases of any, statistics. For the economy the author relies heavily on the federal government's *Second Progress Report on the National Development Plan, 1970-74*, whose tables are copiously reproduced, supplemented by a newspaper survey of retail prices. These give a numerical value for at most four of the fifteen indicators. The remaining areas are not open to precise statistical assessment, and so the views of various groups are inferred from the letter columns, editorial comments, and reported speeches in two Nigerian newspapers, the *Daily Times* and the *New Nigerian*, supplemented by some surveys carried out by the author in 1972. (These are more fully reported in his *Societal Reconstruction in Two African States*.) He concludes that there was considerable dissatisfaction with the performance of the military regime among all the groups named, even among the leading soldiers. This was dramatically confirmed by the military coup which toppled Gowon in July 1975, an event which is briefly noted in a postscript.

The author's purpose is to produce a case-study of a single military regime, viewed from various vantage points, and he keeps strictly to this aim. The first chapter contains some brief and pertinent criticisms of recent writings on the military in government, but he does not elaborate any new theories or conceptual schemes of his own. He declines to compare the performance of the Gowon regime with any contemporary military government, or with the preceding colonial and civilian regimes, pointing out that such comparisons would be invalidated by the recent vast increase in Nigeria's oil revenues. The furthest he goes in specific comparison is to juxtapose the priorities in capital investment allocations of the 1962-68 civilian plan and the 1971-77 military plan, which mainly highlights increased defense expenditure. Otherwise the soldiers' performance is almost entirely assessed against their promises and professed aims, and against the expectations of the population. Consequently the book is largely a chronicle of rising consumer dissatisfaction with

the military in power which, like most governments in the world whether civilian or military, failed to fulfill its manifestoes.

Within the limits he has set himself, the author provides a broad conspectus of Nigerian opinion on Gowon's government and illuminates why it succumbed so easily. The book is clearly written and admirably free of jargon.

N. J. MINERS

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ASIA AND THE EAST

RHODAS MURPHEY. *The Outsiders: The Western Experience in India and China*. (Michigan Studies on China.) Ann Arbor: University of Michigan Press, for Center for Chinese Studies, University of Michigan. 1977. Pp. ix, 299. \$16.50.

There was a time when historians could speak with some assurance about the importance of the Western impact on Asia. The West's intrusions there, research seemed to say, were touching off and giving shape to modern Asian national movements. Within the last decade or so, however, the Western impact theory has been challenged by research that has stressed the role of Asians as shapers of their own destinies. Sinologists have been particularly vigorous advocates of this newer approach. They can point to evidence that China's revolutionary upheaval was produced largely by internal forces and that deeply-rooted native traditions are serving as prime guides for the emerging Chinese nation.

Rhoads Murphey is generally sympathetic to these revisionist efforts, but he is sharply critical of the results. The new generalizations have all too frequently been propounded as if those who embrace them had discovered a universal truth. Yet much of this theorizing is in fact based on extremely limited evidence—a single Asian society, a narrow span of time, and a restricted list of topics. Murphey hopes to avoid these distortions by synthesizing a vast amount of specialized research on India and China and by identifying those concepts that can be sustained by a broad reading of the evidence. He picks up the narrative of Asian history in the eighteenth century; ranges over numerous topics in social, economic, and political history; and, so that the diversity of Asia's experience might be fully represented, compares India and China.

Despite a subtitle that suggests otherwise, little space is given to a review of the "Western experience" in Asia. Murphey contends that, viewed broadly, the patterns of Western intrusion and conduct were much the same everywhere. It was

the particular Asian setting, rather than the variety of imperialism, that determined whether there would be an Asian response. Herein, he contends, one finds the root of India's and China's diverging experience.

British colonialism in India was fostered by native elites who were more impressed by what their Western conquerors had to offer than by what they found in their own traditions. In consequence, the urban enclaves, at first dominated by the British, became the source of ideas and institutions for reshaping Indian society. According to the author, Japan and the states of Southeast Asia, could be shown to have responded similarly to contacts with the West.

China, however, remained virtually impervious to foreign intrusions. Murphey finds persuasive the contentions of Sinologists who hold that what went on in the treaty ports was essentially peripheral to China's political, social, and economic development; there were Chinese advocates of Western models for a new China, but these were ultimately discredited.

In short, Murphey upholds a substantial part of the Sinologists' argument that the old Western impact/Eastern response theory does not apply to China. Yet, he also finds that some tend to push their contentions too far. Sinologists are wrong when they exclude the West altogether from China's revolutionary history. If Western imperialism was incidental to the making of the Chinese revolution, China *has* drawn freely from the West while attempting to reassemble its civilization.

One comes away from *The Outsiders* with the impression that in matters of form the book is not all that Murphey originally intended. There are hints that he may have planned to say more about Japan and Southeast Asia, thus broadening the basis for comparative analysis. Earlier publication of two key chapters has apparently led to some problems in structuring the narrative; there are an extraordinary number of repetitions for a book this size. It is also a bit ironic that a book which criticizes others for generalizing beyond their evidence indulges in some rather sweeping statements of its own. Murphey seems curiously apologetic in his introduction for allowing the manuscript to be printed. Though his reluctance is understandable, it is fortunate he surmounted it. Whatever its deficiencies, *The Outsiders* will have to be examined seriously by every student of modern Asia or Western imperialism. It is a book that promises to stimulate the rethinking of cherished ideas.

BURTON F. BEERS

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JEANETTE MIRSKY. *Sir Aurel Stein: Archaeological Explorer*. Chicago: University of Chicago Press. 1977. Pp. xiii, 585. \$17.50.

Aurel Stein published his first scholarly article on Asia in 1887. He died in Kabul in 1943, at age 80, planning yet another expedition. The intervening years were intensely rich in adventurous and productive travel and writing. The *corpus Steinum* is immense, flowing in page after page of unhurried prose—thick paper, profuse illustrations, loving cartography. No other man knew and described Chinese Central Asia with such intimacy, to say nothing of his Indian and Persian studies.

Jeanette Mirsky has ably recaptured the atmosphere of Stein's life and journeys, making excellent use of the extensive correspondence she has unearthed as well as his published work, formal and informal. The result is a detailed personal portrait of a Victorian bachelor antiquarian explorer (all the more British for his Hungarian origin and belated naturalization), dependent upon sentimental ties to distant friends and family, but happiest upon the trail, eager to record himself as first Western traveler over this pass or entering that oasis, always hunting the source of a White Jade River or mastering a Mountain of Blinding Darkness. If at times the author's style is a bit breathless, overburdened with rhetorical questions, it is not out of character with her subject, and the book is eminently readable.

As a historical study, it is less successful. Mirsky is not blind to Stein's failings: his jealousy of rivals (Sven Hedin, for example), or his possessiveness toward Chinese Turkestan, which made a failure of his last Central Asian expedition when the rulers of Nationalist China were found to hold a different view. His determined and successful pursuit of the "tracks" of Alexander the Great or Hsüan-tsang, or of the outward expansion of Indian Buddhism, came at considerable cost, for he ignored the importance of both the Chinese culture and the Neolithic remains which he encountered along the way. But Mirsky offers no systematic evaluation of Stein's work, archeological, geographical, or historical, and is generally satisfied to chronicle his findings and accept his own estimate of their importance. Nor does she attempt to evaluate Stein's writings: it is indicative that for all its length the book contains no bibliography of Stein's works. Even the journeys can be followed only with additional atlas material; the few maps are mostly borrowed from the works of others and are of little relevance—ironically, for as the years passed, Stein's archeological work gave way to absorption in historical topography.

Mirsky has produced a warm and sympathetic study, and there is surely no need for further de-

scription of Stein's fascinating travels. She has, however, fallen prey to "biographer's syndrome." Her gentle admiration, fulsome in its colorful detail, sparing in deeper judgments, makes for the sort of book Stein himself might have written.

BRITON C. BUSCH
Colgate University

JING-SHEN TAO. *The Jurchen in Twelfth-Century China: A Study of Sincization*. (Publications on Asia of the Institute for Comparative and Foreign Area Studies, number 29.) Seattle: University of Washington Press. 1977. Pp. xiv, 217. \$11.00.

The Jurchen state of Chin (1115–1234), which constitutes the historical link in Northern China between the Khitan state of Liao and the Mongol Yüan dynasty, is of exceptional interest for studying Chinese attitudes toward foreign conquerors and the latter's changing policies toward their Chinese subjects. The Chin state also marks the first appearance of a Tungusic people as a dominant factor in history. Until recently, relatively few monographs on the Chin state and its characteristics were available in Western languages, most of them in Russian and German. Jing-shen Tao's book is therefore of particular importance. He has centered his research on the gradual and tortuous absorption of the Jurchen ruling class into the Chinese cultural and governmental orbit. His starting points are the various theories on the assimilation of foreign conquerors proposed by scholars like Wittfogel, Eberhard, and others. He describes the origin of the Jurchen people in Manchuria and their mixed economy based on farming, hunting, and fishing, which differed basically from the nomadic background of both the Khitan and the Mongols.

After a succinct survey of the Jurchen's rise to power and their conquests in Northern China, Tao outlines the phases of Jurchen rule, distinguishing a period of dualism (coexistence of native and Chinese structures, 1115–50), and the various attempts to transform the Chin state into a state with dominant Chinese traits (culminating in the 1140s and 1150s). With great care and a wealth of details he describes the governmental structure of the Chin state and its methods of personnel recruitment. Tao also relates the attempts of Emperor Shih-tsung (ruled 1161–89) to preserve the native cultural and linguistic heritage of the Jurchen people and to maintain their national identity against the Chinese ways of life. These attempts, however, led only to a temporary equilibrium, and the last decades of the Chin state are characterized by ever-growing sincization. After 1200, the Chin state had, in fact, become a

Chinese state with a ruling class of mixed origins: Jurchen, Chinese, Khitan. When the Mongols annihilated the remnants of the Chin in 1234, Chinese and Jurchen intellectuals transmitted the Chinese cultural heritage in Northern China through a period of destruction and chaos to the newly emerging Mongol state. These intellectuals could rightly consider themselves guardians of Chinese values and traditions.

Tao's book overflows with information. It is profusely annotated and has a very full bibliography. The combination of rich factual detail with interpretive analysis is impressive. There can be no doubt that the book is a contribution of lasting value to our understanding of a crucial period in Chinese history.

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A. A. BOKSHCHANIN. *Imperatorskii Kitai v nachale XV veka (Vnutrenniaia politika)* [Imperial China in the Early 15th Century (Internal Policy)]. Moscow: Izdatel'stvo "Nauka," Glavnaia Redaktsiia vostochnoi literatury. 1976. Pp. 322. 1 r. 44 k.

For a long time, the question of Ming international relations in general, and the famous Cheng Ho voyages in particular, have all but monopolized the attentions of scholars in the West working in Chinese history of the early fifteenth century. Until the recent appearance of David Chan's *The Usurpation of the Prince of Yen* (1975) and Edward Farmer's *Early Ming Government* (1976), little was available regarding the concurrent situation on the home front. As yet, hardly any work exists that tries systematically to relate internal conditions to the remarkable international role that Ming China played under the Yung-lo Emperor (r. 1402–24).

Soviet writing on this period of Ming history seems to be following pretty much the same developmental sequence. In 1968, A. A. Bokshchanin published a book entitled *China and the Countries of the Southern Seas, 14th–16th Centuries* (in Russian) that for the first time gave the Soviet reading public a detailed view of Ming overseas expansionism. Now comes his present offering, which focuses itself upon domestic affairs, and attempts to assess the strengths and weaknesses of the "so-called prosperity" at home that made possible Yung-lo's energetic policies abroad.

There is nothing in a Western language that approaches the broad scope of this work. Bokshchanin starts his study in 1398 when the Ming founder died, leaving as his successor an inexperienced young grandson who tried, but failed, to crush his princely uncle and his large military machine posted on China's northeast border. He ends it with the year 1425, when that uncle, who

usurped the Ming throne and became the Yung-lo Emperor, passed away and his heirs decided to put an end to most of his foreign commitments. The book thus covers two distinct sub-periods—the civil war of 1399–1402 that brought Yung-lo to power, and the generally peaceful years that followed. Bokshchanin details policy and activity almost entirely from the central, or imperial, point of view. For the post-1402 period, the "Veritable Records" of the Yung-lo reign are his main source for an exhaustive treatment of, 1.) the emperor's relationships with the imperial princes, 2.) military administration, 3.) imperial control of the bureaucracy, 4.) imperial fiscal policy, 5.) imperial regulation of trade and manufactures, and 6.) the suppression of "popular movements" and the control of ethnic minorities.

His conclusion is that Yung-lo did manage to "stabilize" the country politically, but that far from inaugurating an era of prosperity, the emperor laid the foundations for a prolonged crisis that "in the end led to a new and grandiose peasant war [the rebellions of 1627–46], the fall of the dynasty, and the establishment in the country of the power of the Manchu conquerors" (p. 304). He sees Yung-lo's power as based neither on the princes, whose cause he had defended, nor on the army, which won him the civil war. Instead of following the Ming founder's techniques of punitive terror plus social and political reform, Yung-lo doubled the size of the bureaucracy, and to control it, created alongside it his own personal apparatus of inner-palace eunuchs. By moving the capital to Peking, he strengthened the backward and "feudalistic" north at the direct expense of the progressive and commercially developed south. In effect, Yung-lo established his own power and stabilized the country by aligning himself with the large landholders and other high "feudal" interests, a policy which placed increasingly heavy rent and tax burdens on the masses and eventually (two centuries later) brought about the fall of the dynasty. This is an interesting thesis, but in the absence of detailed studies of the actual impact of Yung-lo's central policies on China's varied regions and localities, it is difficult to know what to make of it.

In addition to the basic chronicles and other sources, the author has consulted a wide range of recent Chinese scholarship on the period, including contributions from Hong Kong and Taiwan as well as from the mainland. His list of Western writing is also a long one, but he does omit Harold Kahn's *Monarchy in the Emperor's Eyes* (1971), which is pertinent to the historiography of the Yung-lo usurpation. Like most Soviet Sinologists, he neglects Japanese scholarship altogether.

JOHN DARDESS
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A. S. IPATOVA. *Patrioticheskoe dvizhenie na iuge Kitaia v 40-e gody XIX v.* [The Patriotic Movement in South China in the 1840s]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 206. 1 r. 21 k.

The growing bibliography on social disorders in nineteenth-century China is further expanded by A. S. Ipatova's short monograph. The brief text (pp. 40-168) is based on detailed research in Chinese source materials as well as some materials from Russian archives. The overall attempt, however, to place the Chinese "patriotic" movement around Canton from 1839 to 1849 into an anti-colonial, proto-national-liberation mold seems excessive.

Ipatova's presentation is in three parts. In the first thirty-nine pages she discusses the sources. The Russian material used is from the Central State Historical Archives in Leningrad and from the Archives of the Foreign Policy of Russia in Moscow and includes both the correspondence of the Russian Orthodox mission in Peking and personal papers. Information on the patriotic movement in south China is rather fragmentary in the Russian materials, although they are helpful on the Chinese court at the time (pp. 101-03). The chapter on sources is itself a useful bibliographical essay for those without research specialities in the field. Chapter two covers the disorder against the background of the diplomatic and military events of the Opium War. The last chapter chronologically examines the period 1842-49, which ends with the successful Chinese attempt to keep the English out of Canton—an effort made possible, according to Ipatova, by the support of the movement.

The last three sections of the third chapter are more analytical than chronological and are the most interesting, as well as controversial, of the work. There the author argues that the patriotic movement is best characterized as "the first organized anti-colonial movement in Chinese history" and that it constituted an "early feudal stage of the national-liberation struggle" (p. 144). It is then the beginning of the Chinese people's struggle "against the enslavement of China by foreign capitalists and then by imperialists" (p. 167).

The argument is weakened, however, by the author's own evidence. For example, in her analysis of the social basis of the movement—a useful section for its discussion of the occupational groups involved—she dismisses Frederic Wakeman, Jr.'s suggestion (*Strangers at the Gate*, [1966]) that the commercial people of the Canton area became involved because of trade problems but admits (p. 156) that the merchants (*kuptsyi*) supported the movement against the British only when their interests were threatened. The assertion that the commercial boycott was a special weapon in the

arsenal of Chinese merchants' conscious anti-colonial struggle is strained regardless of the uses to which the boycott was later put.

Ipatova's work is most useful for details of the social disorder around Canton in the 1840s, and especially, for a detailed analysis of the proclamations issued by the movement; however, the attempt to place the movement into a larger historical perspective is overdone.

R. EDWARD GLATFELTER
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WELLINGTON K. K. CHAN. *Merchants, Mandarins, and Modern Enterprise in Late Ch'ing China*. (Harvard East Asian Monographs, number 79.) Cambridge Mass.: East Asian Research Center, Harvard University; distributed by Harvard University Press, Cambridge. 1977. Pp. xiv, 323. \$15.00.

This is not just another descriptive account of Chinese "modernization"; it is a perceptive investigation into that complex process which concerns fundamental issues in the reorientation of a traditional society. Wellington K. K. Chan draws upon primary sources and scholarly publications that have been accumulating during the past quarter century. His interpretive monograph is a welcome sign that the Western study of Chinese social history has progressed toward maturity since it was first given serious attention some decades ago.

The scope and main thesis of the book are eloquently stated in the "Introduction"—that a reappraisal of the status and role of the merchant class is essential for understanding the modernization of economic enterprises in late nineteenth- and early twentieth-century China. The author proceeds from a variety of vantage points in the three main sections of the book. Part one, "The merchant class: changing perspectives," examines the shifting realities of the merchant's position in relation to the political power structure in late-traditional times. Chan reminds the reader that the myth of the degradation of the merchant class does not always coincide with fact; indeed, from the Sung period on, legal liberalization of merchant status did not keep pace with the faster rate of social change brought about by increased mercantile activities. When re-examination of the situation did take place it was by scholar-officials in the late nineteenth century—a period when Western power confronted China—thus injecting the issue that underlay the problems and policies discussed in the rest of the book, that of national purpose vis-à-vis private economic interests.

Part two, "The provincial government: initiative and dominance," depicts the multifaceted interactions between economic interests and bureaucratic power within the context of the (generally

accepted) national goals of "wealth and power." The author's special contribution here is his view of internal factors that led to the adoption of such policies as *kuan-tu shang-p'an* (official supervision and merchant management) and *kuan shang ho-p'an* (official-merchant joint management) from the 1870s onward; in the end, the bureaucratic element representing provincial administrative authority dominated the modern enterprises. Citing case examples to illustrate the emergence of "official entrepreneurs" and the "merchant and gentry in private enterprise" (chapters six and seven), Chan not only vividly reconstructs the blurring of merchant and official worlds, but also explains why and how these compromise policies did not establish the framework for a hoped-for modern China.

Part three, "The central government: an unsuccessful challenge," analyzes Peking's effort after 1900 to assert central authority in directing modern enterprises. The functionally specialized Ministry of Commerce was founded in 1903, and a system of government awards was devised to encourage capital investment. Here too, however, the crisscrossing of inherited interests and values with new needs, goals, and techniques often brought about unexpected effects.

The inner dynamics of a culture are ultimately the primary determinants of the direction of societal development. In making this worthwhile contribution to the study of China and its efforts to meet the challenges of the modern world, Wellington Chan has also pointed to areas in economic and social history where further research should prove fruitful. This book is useful both to specialists on modern China and to those interested in the general historical processes of cultural and institutional adaptation and change.

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HUGH TREVOR-ROPER. *Hermit of Peking: The Hidden Life of Sir Edmund Backhouse*. New York: Alfred A. Knopf. 1977. Pp. 316. \$10.00.

For a number of Westerners in the early decades of this century, Peking provided an ambience for eccentric behavior. Hugh Trevor-Roper has exhumed one extraordinary figure from old Peking in the person of Sir Edmund Backhouse (1873-1944).

After his arrival in Peking in 1898, Backhouse established his credentials as a Sinologist and as an eccentric recluse. For several years, he worked as an assistant to the famous correspondent of the *Times*, G. E. Morrison, who relied on his linguistic ability and general familiarity with the Chinese scene. By 1910, Backhouse had collaborated with J. O. P. Bland, another *Times* correspondent, to produce *China under the Empress Dowager*, one of the

standard references for the life and times of Tz'u-hsi. Four years later, the two published another major work on modern Chinese political history. In the decade following the fall of the Manchus, Backhouse added to his fame by donating thousands of Chinese books and manuscripts, including rare and valuable editions, to the Bodleian Library at Oxford, making its collection one of the finest in Europe.

Few contemporaries suspected that the cultural elasticity which had enabled Backhouse to expand his life into a Chinese context was complemented by a moral elasticity that involved him in attempts to deceive or swindle several Western firms in China, as well as the British government (which had hired him as a secret agent in 1915) and even the Bodleian. Backhouse had an uncanny ability to escape the consequences of his illicit activities, so that his scholarly reputation remained more or less intact until the appearance of this book.

A two-volume manuscript of memoirs, passed to him in 1973 under somewhat mysterious circumstances, first attracted Trevor-Roper's attention to Backhouse. These memoirs revealed Backhouse's secret life and elucidated in "pornographic" detail his sexual encounters with many of the most prominent men of Europe and even with the septuagenarian Dowager Empress, Tz'u-hsi! Incredulous, the author began to gather information on Backhouse, soon discovering that he had stumbled onto a mystery man whose life appeared more astonishing as each new facet came into view.

In reconstructing Backhouse's impressive accomplishments as a liar, as well as his fraudulent activities, the author argues that the so-called Ching-shan Diary, a chapter in Backhouse's first book and long considered a fake, was forged by the Sinologist himself. This conclusion seems premature, though it is clear that Backhouse knew that the diary was not authentic. Since the author's case is circumstantial, the question remains whether Backhouse, even with his linguistic brilliance and calligraphic skills, was capable of such a masterful job of forgery in literary Chinese written in the cursive style. Trevor-Roper perhaps forgets that the Chinese, too, enjoy a great tradition of literary forgery.

What were Backhouse's motivations? The author's attempt at psychoanalysis in the concluding chapter, though provoking and illuminating, will doubtless leave unanswered questions for many readers. It is clear that financial gain was not the major objective of his hoaxes. Instead, he enjoyed the psychic rewards of hoodwinking his fellow Britons, whom he detested, or gauche Americans.

In this enormously entertaining book that reads more like a detective story than a biography, Trevor-Roper treats us to a delightful excursion into Backhouse's world of fiction and fact to see how

the bogus and authentic were cleverly interwoven by a wizard of deception.

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YEN CHING HWANG. *The Overseas Chinese and the 1911 Revolution: With Special Reference to Singapore and Malaya.* (East Asian Historical Monographs.) New York: Cambridge University Press. 1976. Pp. xxvii, 439. \$26.50.

Of the several revolutions in modern China, the anti-Qing republican movement was unique in that it took place to a large extent abroad, which may be why its accomplishments were so meager. The role of Japan in the movement is generally understood. That of Southeast Asia, however, is scarcely known, though it was hardly less crucial than that of Japan. Yen Ching Hwang's is the first scholarly study in English of the Chinese revolutionary movement in Singapore and Malaya, the twin centers of activities in Southeast Asia. It is a well researched and thoroughly documented work based on the local Chinese newspapers of the time and on the Kuomintang archives in Taiwan. It gives a particularly interesting description of the 1908-10 newspaper duel in Singapore between the revolutionary *Zhong-xing ri-bao* and the reformist *Nan-yang zong-hui bao*, which was a continuation of the earlier, and better known, polemic in Japan between the *Min bao* and the *Xin-min cong-bao*. It also has a good account of Sun Yat-sen's counter-attack on his factional opponents within the *Tong-meng hui* during 1908-11. Students of the 1911 revolution will find the book profitable reading.

While strong on documentation, Yen's study is weak on analysis. Its interpretation conforms without deviation to the orthodox version purveyed by both the Kuomintang and the Communists. The revolution is thus seen as the direct accomplishment of the revolutionaries, who are considered synonymous with Sun Yat-sen and his followers. As a result, revolutionaries not associated with Sun, such as Liang Qi-chao before 1903 or Tao Cheng-zhang after 1907, are practically ignored, even though both apparently enjoyed substantial support among the Chinese in Singapore and Malaya. Similarly, no serious consideration is given to the possibility that the revolutionary movement, instead of proceeding from triumph to triumph (as Yen implies), may have suffered a severe setback in the last years of the post-Boxer decade. Yen's own findings—viz., the general observance of the day of mourning for the Guang-xu Emperor in November 1908, the demise of the *Zhong-xing ri-bao* in 1910, the refusal of the Singapore *Tong-meng hui* to contribute to the Canton revolt of April 1911—

all suggest that the revolutionary movement declined after 1908. Yen generally fails to confront this refractory evidence, and when he does try to explain, the explanation is quite lame and is not supported by any evidence.

Yen also pays inadequate attention to the influence of ethno-linguistic differences upon the revolutionary movement. The Chinese community in Singapore and Malaya was composed of several distinct language groups. Yen concludes that one consequence of the revolutionary movement was a growing unity within the community as "people from different dialect groups were gathered together to work for the revolution." This finding is largely unsubstantiated, however, and again the evidence suggests otherwise, as when the Singaporeans evidently sided with their fellow Teochew Xu Xue-qiu against the Cantonese Sun Yat-sen.

EDWARD RHOADS
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Austin

ERNEST P. YOUNG. *The Presidency of Yuan Shih-k'ai: Liberalism and Dictatorship in Early Republican China.* (Michigan Studies on China.) Ann Arbor: University of Michigan Press, for the Center for Chinese Studies. 1977. Pp. viii, 347. \$17.50.

This is a book about Yuan Shih-k'ai's policies as president of China between 1912 and 1916. Ernest P. Young is not particularly concerned with the shortcomings of Yuan's character, about which so much has been written, but tries to show how Yuan conceived China's problems, and the inherent contradictions in his attempts to resolve them.

Young argues that Yuan sought centralization of national power, which he considered essential if China were to resist foreign pressures and to effect the reforms necessary to modernize Chinese society. It was that need for centralization that led Yuan to move against the Kuomintang, set aside the constitutional organs of the republic, and establish a dictatorship in 1913-14.

Yuan failed because his conception of centralization was so narrow. He thought largely in administrative terms, employing old and new methods of control to subordinate officials and to restrict popular participation in politics. Yuan's administration had no firm basis in any sector of Chinese society. The traditions, education, and political activities of the gentry before and during the 1911 Revolution made it the logical class to support Yuan's government, but gentry interests were strongly provincial, and thus ran counter to the centralization that Yuan thought essential. Yuan's policies often irritated the merchants, who did not think he represented their interests. To have sought mass support to compensate for the alien-

ated gentry or remote merchants had such revolutionary social implications that Yuan never even considered it. Therefore he sought the goals of Chinese nationalism through administrative centralization, not social mobilization.

Yuan's policies in some respects were similar to those of Chiang Kai-shek during the Nanking Decade, a similarity that Young discusses. But, like Chiang's approach, Yuan's programs needed time and popular patience to work, if there was any possibility they could work at all. Those elements were not available where imperialist aggression was accelerating, and Chinese nationalists ceaselessly demanded effective action against it.

In developing the cogent analysis inadequately sketched above, Young draws from a variety of Chinese and Japanese sources to provide meticulously documented detail about Yuan's political maneuvers, his relationships with foreigners, and the reactions of Chinese to Yuan's policies. Young offers the most thorough picture yet of the texture and tone of Yuan's presidency. He also offers stimulating comparative observations.

This book has been gestating for a long time, and it is a pleasure to report that it is well worth the wait; thoroughly researched, compellingly argued, written in a modest and lucid style, it is a model of splendid scholarship.

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PARSHOTAM MEHRA. *Tibetan Polity, 1904-37: The Conflict Between the 13th Dalai Lama and the 9th Panchen.* (Asiatische Forschungen, number 49.) Wiesbaden: Otto Harrassowitz. 1976. Pp. 94. DM 30.

The study contained in this slender volume of some ninety-four pages is based primarily on archival data from the British Foreign Office, the India Office Library, and the National Archives of the Government of India in New Delhi, with additional information from secondary sources and biographical works. The main body of this text consists of an introduction reviewing the historical relationships between the Dalai Lama and the Panchen Lama, an assessment of Tibetan polity, 1904-37 presented under fourteen subtitled time periods, and an epilogue which summarizes events relevant to the present Dalai Lama and the tenth Panchen Lama from 1937 to the aftermath of the 1959 Tibetan revolt.

In general this study clarifies the actions of the thirteenth Dalai Lama and the ninth Panchen Lama and their entourage which initially created antagonism between them and subsequent efforts to reconcile them. Archival documentation on the

tactical maneuvering and strategic policies of neighboring governments, particularly those of British India and China, places the conflict between the two incarnate lamas in a broader political perspective.

The assessment of Tibetan polity in the first half of this century is perceptive and reliable; however, the historical review given in the introduction contains some misleading or erroneous statements. It is stated that "... Chen-re-si, Jam-pe-yang ... and Do-je-chhang (Holder of the Thunderbolt) constitute the trinity of Tibet's all-powerful deities" (p. 2) and that the first is incarnate in the Dalai Lama, the second in the Ch'ing Emperors of China, and the third in the Panchen Lama (p. 3). Such a trinity wrongly mixes a Buddha with bodhisattvas. Chen-re-si [Tibetan: Spyan-ras-gzigs] and Jam-pe-yang [Jam-dpal-byangs] are bodhisattvas, traditionally believed to incarnate in the human forms noted above. The third bodhisattva of the proper trinity is Cha-na-do-je [Phyag-na rdo-rje]; not Do-je-chhang [Rdo-rje-'chang] who is regarded by the reformed sects of Tibetan Buddhism as the Adibuddha. Moreover, the Panchen Lama is regarded as an Incarnation of Ö-pa-me ['Od-dpag-med], the Buddha of Infinite Light.

Contrary to footnote 5 on page 3, the monastery of Drepung was *not* founded by Ge-dün Trub-pa [Dge-'dun grub-pa]; but by Jam-yang chö-je [Jam-dbyangs chos-rje]. Ge-dün Trub-pa, traditionally regarded as the first Dalai Lama, founded the monastery of Tashilhunpo near Shigatse, which became the see of the Panchen Lamas.

The statement that politically Tibet was still under the sway of its "... Karma-pa chiefs who patronized the older, Red Hat, sect" (p. 4) should read: "... Tsang chiefs who patronized the older Red Hat Karma-pa sect."

Notwithstanding its minor errors, this study is a valuable contribution to the field of Tibetan political history.

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IAN NISH. *Japanese Foreign Policy, 1869-1942: Kasu-migaseki to Miyakezaka.* Boston: Routledge and Kegan Paul. 1977. Pp. xii, 346. \$12.50.

This is a comprehensible and readable survey of Japanese foreign policy between 1869 and 1942. Ian Nish, whose writings on the Anglo-Japanese alliance have made him one of the world's foremost authorities on Japanese diplomatic history, here adopts a biographical approach. He chronicles the course of the Foreign Ministry's policy formulations and implementations in terms of the personalities, backgrounds, and ideas of twelve key lead-

ers: Iwakura, Mutsu, Aoki, Komura, Katō, Ishii, Shidehara, Tanaka, Uchida, Hirota, Konoe, and Matsuoka.

It would be difficult to quarrel with the choice of the twelve men in the book. They do represent Japan's foreign policy establishment during the period under consideration. Nish's analysis of the foreign policies of the last six men—in other words, those who were active after the World War—is on the whole more interesting and imaginative than that of the first six. This may reflect the fact that a great deal more is known about the pre-1918 than the post-1918 period and that it is exceedingly difficult, thanks to the existence of excellent monographs (Nish's included), to be original about the first period. Historians' concerns recently have shifted to the years after the war, especially the 1930s, and this undoubtedly is reflected in the book's detailed and useful treatment of the decade. Nish traces the gradual erosion of the Foreign Ministry's power in Japanese decision-making and harshly judges the ineptness of Foreign Minister Hirota, among others. Even so, Nish reminds the reader that Foreign Ministry officials were not meek supplicants to the powerful army, but that some of them tried in a number of ways to regain control over crucial decisions.

The story ends with the outbreak of the Japanese-American war. This is curious insofar as the author keeps stressing that one should not view the course of Japanese diplomacy during the 1930s in terms of "the road to Pearl Harbor." For this reason, he does not dwell much upon the United States-Japanese negotiations during 1941. Nor does he depict in detail the personality and ideas of Foreign Minister Tōgō, probably a far more interesting and influential figure than Hirota or Matsuoka. By stopping his analysis in 1941, Nish may give the impression that everything came to a halt with the outbreak of the war. It could be argued, on the contrary, that the Japanese Foreign Ministry had its finest hour during the war, especially during 1944–45, when its officials worked behind the scenes to bring the war to an end. Tōgō played a pivotal role in the story. From such a perspective, it is possible to assert that there was more continuity in the history of Japanese foreign policy between the 1920s and the 1940s than the book assumes.

The book concludes with a useful appendix, a translation of several important Japanese documents. These documents are particularly valuable to non-specialists as examples of the thoughts of Japanese leaders and the idioms they used to express themselves.

AKIRA IRIYE
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E. PATRICIA TSURUMI. *Japanese Colonial Education in Taiwan, 1895–1945*. (Harvard East Asian Series, number 88.) Cambridge, Mass.: Harvard University Press. 1977. Pp. xiii, 334. \$20.00.

E. Patricia Tsurumi's work is a serious monograph, excellently written, organized, and argued. It deals with the importance of education as an instrument of Japanese colonial development in Taiwan and discusses the psychological consequences of Japanese rule on the Taiwanese. It will be useful to students of comparative studies of colonialism, as well as to those interested in modern Japan and Taiwan.

In formulating concepts and programs for Taiwan the Japanese colonial administrators drew heavily on the philosophy and experiences of the educational system of Meiji Japan. However, unlike in Japan where emphasis was balanced between higher education and primary education, "Japanese education in Taiwan was to be mainly elementary education; it was to train the Taiwanese for life and work in a new world; and it was above all to make them unquestionably loyal to Japan" (p. 11).

During the first half of the Japanese administration, the colonial rulers pursued a policy of school segregation between Taiwanese and Japanese in Taiwan. After 1920, as a result of the adoption of an integration policy that aimed at eventually assimilating the Taiwanese and firmly incorporating the island into Japan, school segregation was brought to an end. Ironically, school desegregation actually contributed to the decline of educational opportunities for the Taiwanese at the post-primary level due to the widespread practice of favoring the admission of one's own nationals over colonial subjects. Tsurumi points out that such a practice was the norm of an overseas territory.

Viewing higher education (especially in law and politics) for the ruled as posing a potential threat to the colonial regime, the Japanese rulers developed high-schooling for Taiwanese slowly, sparingly, and with extreme reluctance. They channeled irresistible Taiwanese demands for higher education into professional studies which would produce the kind of trained natives the colony required. Consequently, educated Taiwanese were usually prepared for careers in medicine, pedagogy, applied science, or commerce. Even so, before the end of Japanese rule, thousands of Taiwanese had studied such subjects as law, political science, economics, literature, arts, philosophy, and natural science mainly in the colleges and universities of Japan.

The author compares and contrasts the reac-

tions of the Taiwanese and the Koreans to Japanese rule in general, and to schooling in particular. She also discusses the way in which various groups of Taiwanese were influenced by Japanese education. "Public education did much to reconcile the Taiwanese to the Japanese colonial government and the institutions it introduced. Generally, the more Japanese education a Taiwanese received, the more assimilated she or he tended to be. At the same time, it was among Japanese-trained intellectuals that an anticolonial movement against Japanese rule originated. But a large segment of this movement was extremely moderate, with leaders who worked completely within Japan's legal and governmental structures. In sharp contrast, the Japanese schools of Korea turned out class after class of anti-Japanese militants" (p. 161).

Tsurumi argues convincingly that, with the exception of America in the Philippines, no other colonial power approached native education with the seriousness of purpose that Japanese educators displayed in Taiwan, and that the overall record of Japanese elementary education is impressive. She also indicates that life in Taiwan, under half a century of Japanese rule, became closer to life in the mother country than was the case in any colony under Western administration, and that Japanese advocacy of schooling for girls and women significantly contributed to the enhanced status of women in Taiwan.

CHING-CHIH CHEN
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GEORGE B. BIKLE, JR. *The New Jerusalem: Aspects of Utopianism in the Thought of Kagawa Toyohiko*. (The Association for Asian Studies. Monograph number 30.) Tucson: University of Arizona Press, for the Association for Asian Studies. 1976. Pp. 343. Cloth \$8.95, paper \$4.95.

Forty years ago Kagawa Toyohiko was the best known Japanese outside his own land and a famous author at home. Now he is practically unknown abroad and Japanese show little interest in his work. The challenge of the *Complete Works* (24 volumes, 12,000 pages) has gone unanswered by non-Japanese scholars since their publication in 1963-64. The work of George B. Bikle, Jr. summarizes many of Kagawa's publications from the *Complete Works* in support of a convincing thesis.

Bikle finds that the thread which ties together the enormous variety of Kagawa's activities is rational social planning given shape by mystic contemplation. Kagawa was above all a Christian clergyman preaching each week and cooperating

in mass evangelical campaigns. At the same time, he went far beyond other representatives of the middle-class church in his concern for the economically disadvantaged. Bikle considers Kagawa's unique contribution to have been his adoption of the guild socialism of G. D. H. Cole, with the corollary that principles of Christian brotherhood must motivate the members of society. To support this thesis, Bikle touches lightly on the dramatic biographical elements which dominate most other studies of Kagawa. Kagawa's youthful identity struggles, his slum reform work and leadership in labor unions are dealt with only to the extent that they contributed to the development of a comprehensive social philosophy. According to this philosophy, guilds and cooperatives form the basis of the state. Kagawa's immensely successful experiments in these areas lent credence to his overall scheme and provoked the opposition of Communists and Fascists who cherished their own and opposing utopian visions. Bikle's concentration on the philosophy and theology which underlay Kagawa's activity tells Western readers for the first time why so many thoughtful Japanese followed him.

At the same time, Bikle says nothing about why Kagawa has fallen from public consciousness since his death in 1960. In following the basic line of interpretation laid down by Yokoyama Haruichi, Sumiya Mikio and the Western biographers, all admirers of Kagawa, Bikle does not deal with the one element in Kagawa which bothers other Japanese students of their own social history. This is his apparent support for government propaganda during World War II. An essay by Ota Yuzo in a forthcoming work (*Pacifism in Japan, the Christian and Socialist Tradition*, University of British Columbia Press) presents this point of view. Considering the development of the field until now, it is probably too much to expect one man to deal with both points of view.

Though unexplained allusions ("Guild Ilium"), cumbersome sentences (up to 90 words) and unusual vocabulary ("emprise," "mergence," "teleofinalism") divert one from Bikle's argument, the patient reader is rewarded by the rich contents and synthesis. This is a major contribution to an important new field.

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CHRISTOPHER JOHN BAKER. *The Politics of South India, 1920-1937*. (Cambridge South Asian Studies, number 17.) New York: Cambridge University Press. 1976. Pp. xxiii, 363. \$28.50.

D. A. WASHBROOK. *The Emergence of Provincial Politics: The Madras Presidency, 1870-1920*. (Cambridge South Asian Studies, number 18.) New York: Cambridge University Press. 1976. Pp. x, 358. \$24.50.

Together, these two books are noteworthy attempts to bring us an understanding of politics in South India during seventy years of the British Raj. One traces political processes within the Madras Presidency down to 1920 and the other continues to trace developments down to 1937. Both explore ways in which elements of a pliable apparatus of government—the administrative structure superimposed upon existing (sometimes ancient) institutions by an imperial authority which was not always completely aware of what it was really doing—could be manipulated if not exploited by indigenous local interests bent on advancing their own particular ends.

D. A. Washbrook's work covers a time of profound transition. Long-sundered and perhaps never previously integrated fragments of political existence had slowly been brought together and bound, by strengthening connections, to the machinery of the state. New forms of political consciousness had produced altogether different, unprecedented kinds of political behavior. Still of central importance in indigenous politics, Washbrook would contend, were patron-client relationships—means by which, incidentally, even the Raj itself had first come to be what it was. But increasingly, there was discontent with the limited role of the Madras government (and its district stations); merely doing what was essential for public peace or culling its share of the local revenues (as "tribute," etc.) no longer sufficed. Politics, "the exercise of power," required institutional foundations. Without a power base, no political will could be imposed or commands enforced.

The role of Western-educated elites should not be exaggerated. Few were free agents acting only for themselves. Most were brokers mediating transactions between local lords or magnates (mainly rural) and various institutions of government. Behind each agent or broker, agitator or organizer, was a locality patron. It was the patron who paid the bills: for court disputes, cultural events, religious movements, temple endowments (and festivals), *zamindari* reorganization, large irrigation works, restructuring of local government, new forms of local self-government, changes in income tax, reallocations of forest privileges, or for founding modern colleges. Only when we understand this, can we understand how agitational politics arose or how decisions of the Madras Legislative Council developed.

Christopher John Baker's work takes up much

the same theme, probing into the politics of the interwar years (1920-37). The Non-Brahman Movement, caste organization, "peasant" movements, and the growth of the Congress to unquestioned power are among the important issues which are examined. Baker also discusses unprecedented stresses that were placed upon existing political structures: new institutions, especially a Madras executive of cabinet ministers responsible to an elected legislature; emergence of "mass" movements, appealing to local and parochial interests and to caste, class, language, and ideology; and rapid expansion of all-India campaigns to expel the British. The oldest, largest, and most neglected province of India came to life as never before. Rulers, confronted with such pressures and with the paradoxes of the imperial system, sought both to implement reforms and to transform the basis of government. Yet, what was still essentially an agrarian society was little prepared for the impact of a world-wide depression. The resulting major political crisis which emerged during the 1930s left inner conflicts which would disturb the country long after the British were gone.

Both works, with their ambitious reach, innovative methodology, and flashes of analytical brilliance, will excite scholarly interest for a long time to come. A thoroughly comprehensive and cohesive treatment of the history of South India during the twilight of the empire has never before been attempted. For this reason alone, the contributions of Washbrook and Baker are important and are highly deserving of attention.

ROBERT ERIC FRYKENBERG
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SEYD HUSSEIN ALATAS. *The Myth of the Lazy Native: A Study of the Image of the Malays, Filipinos and Javanese from the 16th to the 20th Century and its Function in the Ideology of Colonial Capitalism*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1977. Pp. vii, 267. \$22.50.

The basic argument of this book is that the "ideology of colonial capitalism evaluated people according to their utility in their production system and the profit level" under Western rule, with the result that the capabilities of the indigenous inhabitants were denigrated through various myths and stereotypes, notably, the myth of the lazy native. Seyd Hussein Alatas presents many examples of this myth from the writings of colonial administrators, scholars, and travelers from the sixteenth through twentieth centuries. He argues that the frequency of its use increased simultaneously with the "effective period of colonial capital-

ism" from the eighteenth to early twentieth century (p. 2). By "colonial capitalism" Alatas means the pursuit of profit within institutional forms different from those in Western Europe.

Apparently the book began as an examination of the myth in British-ruled Malaya and was subsequently broadened to include the cases of Java under the Dutch and the Philippines under the Spanish; the sections on Malaya predominate. I question the imputed link between colonial capitalism and this myth since the appearance of the latter seems to antedate the former in the Philippines; in addition, the stereotype is often used in all three regions in references to servants, assuredly a non-capitalist category. Perhaps the myth should be seen as a weapon to justify rule over others, whether that domination is largely based on colonial capitalism or not.

Alatas does indicate differences in the use of the myth by colonial elites, but he appears to overlook the very real differences in the nature of colonial capitalism in the three colonies and attributes the remarkable similarity in views of the "native" to "a mode of production which did not experience drastic changes in vast areas of activity" (p. 18). Alatas also makes the startling counter-factual assertion that had "there been no colonialism, by the end of the 19th century there would have been Malay and Javanese trading houses in the West" (p. 21) and argues that Aceh and other Indonesian states would have had histories similar to those of Japan, Russia, and Turkey.

Despite the limitations of this book, it is a major contribution to the comparative study of colonialism worldwide, not only in Southeast Asia. Alatas establishes the similarity and prevalence of the myth, systematically destroys it through an examination of its ideological basis and impressionistic nature, and illustrates its influence in the work of such scholars as Clive Day, J. S. Furnivall, Karl Marx, and some contemporary Malay writers (who receive disproportionate attention). The book also contains valuable discussions of "national character" studies, the "negative influence of capitalism" (labor exploitation, opium use, medical treatment), the adoption of colonial ideology by indigenous elites, the bankruptcy of the plea of "historical necessity" for colonial actions, and the "principle of misplaced responsibility," whereby colonial rule creates a situation for which "native society" is then blamed.

BRUCE CRUIKSHANK
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NICHOLAS P. CUSHNER. *Landed Estates in the Colonial Philippines*. (Monograph Series, number 20.) New

Haven, Conn.: Yale University Southeast Asia Studies. 1976. Pp. vi, 145.

DENNIS MORROW ROTH. *The Friar Estates of the Philippines*. Albuquerque: University of New Mexico Press. 1977. Pp. xi, 197. \$12.00.

From the sixteenth century into the early twentieth century large haciendas, especially those owned by the Roman Catholic orders, were a recurrent source of controversy in the Philippines. They often served as the focus for struggles, both between Spaniards and Filipinos and between church and state. Although there were no such estates in many regions of the country, in the four provinces closest to Manila they came to occupy half of all the land and thus were disproportionately influential in the perceptions of both colonialists and anti-colonialists. Spaniards debated how to regulate them; nineteenth-century Filipino "Propagandists" such as José Rizal used them as symbols of the worst excesses of Spanish rule; and the Americans after 1898 found that the "Friar Lands" had to be broken up if Philippine nationalism were to be co-opted.

Until the publication of these two books, however, the scholarly study of this institution scarcely existed; our knowledge of it lagged at the level of nineteenth-century propaganda. Nicholas P. Cushner and Dennis Morrow Roth have for the first time gone to the archives to uncover how these estates were formed and how they functioned legally, economically, ecologically, and socially. Despite some duplication, the works are essentially complementary. Cushner limits himself to one province (Tondo) and to the period before 1800, with major emphasis on the sixteenth-century origins of the estates and the first half-century of operations. Unlike Roth, he includes secular as well as religious haciendas, although the latter swallowed up the former within a century. Thus, Cushner tends to see the problems as Spanish rather than clerical and pays less attention to tensions between Church and state than Roth does. Roth covers all four Luzon provinces where friar estates flourished and brings the story up to 1898. His greatest strength is his analysis of the operations of the estates during the late seventeenth and eighteenth centuries—what the conditions of tenure were, how large the profits were, which of the alleged "abuses" were probably committed in fact, and why the benefits for tenants sometimes outweighed the liabilities. The sources are also complementary; Roth depends primarily on Dominican archives, while Cushner has more materials on the Augustinians and Jesuits.

Although each author was aware of previous articles by the other, neither benefited from seeing the other's completed study, so the serious Filipino

ist must read both works and attempt to integrate them himself. Even in tandem they are not the definitive study of the haciendas of the Spanish Philippines—such pioneering works always raise as many questions as they answer—but they must serve as the starting point for all further research. If one had to choose a single book on the subject now, Roth would be the choice because of his greater detail and comprehensiveness. His longer chronological framework lets him show how the “problem” presented by the friar estates was redefined over time with shifts in the equation of land, labor, and markets, although he analyzes the factors of population growth and resultant pressure on agricultural land less thoroughly than he might. This diachronic perspective is a salutary corrective to the assumption that nineteenth- and twentieth-century grievances, such as the proliferation of subtenancy and insecurity of tenure on the estates, can be projected back in time. In the 1745 revolt, to which Roth devotes an entire chapter, the issues were usurpation and enclosure rather than conditions of tenancy, and resistance came primarily from neighboring towns rather than from tenants of the haciendas.

It is not clear whether these books will appeal to a much wider audience than Filipinists. Cushner makes little effort in this direction, though he comments briefly and provocatively on the impact of the introduction of Western concepts of land ownership into a non-Western society. Roth, on the other hand, consciously tries to situate his work in the broader field of studies of landed estates, citing many useful parallels drawn from Latin American haciendas and European feudalism. But the Philippines is in Southeast Asia, and the superimposition of European institutions does not turn Malay peasants into Mexican or French ones, nor wet-rice agriculture into corn or wheat. Roth is well aware of this, and his sense of distinctly Filipino values and behaviors is keen, but the resulting synthesis between monographic specificity and more universal theory is often uncomfortable. His conclusion that “the Philippine friar estates combined traditional features of Latin American territorial possession with the high population density characteristic of Southeast Asia” (p. 151) is true enough, but its implications are not self-evident and Roth hardly begins to explore them. As a result, his book at times seems to lose focus, and the sum of the parts seems greater than the whole. Whereas both these works will be essential reading for Filipinists, they will simply remain useful “case studies” for students of landed estates in history.

NORMAN G. OWEN
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BENEDICT J. KERKVLIET. *The Huk Rebellion: A Study of Peasant Revolt in the Philippines*. Berkeley and Los Angeles: University of California Press. 1977. Pp. xvii, 305. \$16.00.

In this account of the rebellion in Central Luzon from the late 1940s into the early 1950s, “the analysis tries to approximate the viewpoint of the villagers who participated” (p. xii). In that Benedict J. Kerkvliet has succeeded. Careful documentary research and interviewing put together a story very much more detailed and consecutive than any before. The sources for the study include captured Huk documents, American government documents from the National Archives and the Federal Records Center, the Roxas papers, Manila newspapers, histories of Philippine municipalities, and twenty-eight interviews listed in the bibliography in addition to others alluded to in the text but not so listed. The presidential office records of the late Commonwealth and early Republic in the Malacanan records *bodega* might provide an additional source, but with relatively little of the viewpoint for which the author cares. Kerkvliet has not aimed to tell the story of the educated, the landed, or the elected, or to adopt several perspectives upon the rebellion. He wishes to tell the story of the “repressed,” and does so with straightforward prose, occasional poignant detail, and a minimum of rhetorical indignation. No need to speak in fury; *res ipsa loquitur*. The art, of course, lies in assembling the *res*.

The story as told is perfectly consistent with the conclusions. Kerkvliet chooses *barrio* San Ricardo to show the deterioration of traditional elite-peasant relationships for ten or twenty years before the Second World War. The peasants thereby lost a generalized social insurance policy. Resulting dislocations and perceived inequities were a major cause of the rebellion. Protest and legal recourse did not lead to improvements. Peasants were rationally motivated to defend themselves against abuses and repression by landlords and governments. They were forced to rebellion, as a last resort, by the blindness and excesses of the privileged.

In Kerkvliet’s view, leadership did not “cause” either unrest or rebellion, but to some degree shaped both. As a corollary, he stresses that the *Partido Komunista ng Pilipinas* did not inspire or control the peasant movement, the *Hukbalahap*, or the Huk rebellion. In this observation he is as far from the Lavas, or Saulo, or Pomeroy as he is from Lansdale or Scaff. He is probably right most of the time, and certainly right much of the time.

One can understand, however, that a book with a hero—the *tao*—must have villains. Kerkvliet says that “both the American and Japanese regimes

were aligned with Filipino elites against poor and propertyless people" (p. 266). Such statements may be arguably held, but seem a bit grand and gratuitous. The effect of that generalization is momentarily and unconsciously to accept a PKP perspective rather than the one promised and largely sustained—of villagers simply loyal to friends and neighbors, struggling for their own survival and dignity.

Kerkvliet ends on the note that living conditions improved little, if at all, after the revolt, and relations between landlord and tenant never returned to the traditional style. The unhappy spectacle presented in the last act is the triumph of mechanized farming in San Ricardo. "Progress," in short, is still another villain. Protest is implicitly but inescapably linked with nostalgia, rebellion with traditionalism, and dignity with failure.

A reader might fairly infer from these pages that the Huk rebellion was a movement of limited reach and diffuse purpose, with theorists weak in vision and commanders flawed in timing. As a rebellion, it greatly amplified earlier themes in the history of Central Luzon, but did not supply a powerful paradigm of change for Filipino society, let alone any other. These, however, are not Kerkvliet's conclusions. Having buried his passion in scholarly discipline, letting it smolder in chosen details, he allows it to spring to flame at the end by quoting a rebel: "No strike, no demonstration, no rebellion fails! Protest against injustice always succeeds" (p. 209). In that statement of faith one believes one has heard the voice of the writer as well, true to his subject, to his introductory promise, and to himself.

THEODORE FRIEND
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TONY GRIFFITHS. *Contemporary Australia*. New York: St. Martin's Press. 1977. Pp. 165. \$14.95.

There has been a marked increase in the volume of writing on contemporary Australian history during the last decade. Most of it falls into one of three categories. There are detailed studies on particular topics, mostly in the form of dissertations and based on the necessarily limited amount of original material publicly available. There have been one or two more ambitious attempts to construct recent history in some analytical depth, based on such detailed original work. And there is a great deal of more superficial writing, ranging from that of journalists with an "inside" view to preliminary surveys of the field.

This volume by Tony Griffiths falls into the last of these categories. It seems to be aimed at university undergraduates or senior high school students.

It covers the period 1939 to 1975 in narrative fashion. The arrangement of chapters is chronological rather than thematic, and there is no serious attempt to analyze or explain major events or policies of the period.

Griffiths' approach to the period is to concentrate upon the political surface rather than to discuss underlying cultural, demographic, or even economic factors. The result is to give his narrative pace, of a somewhat journalistic sort. He offers judgments from time to time, sometimes sensible, occasionally by the use of weighted adjectives, but rarely on the basis of sustained argument. They usually turn out to be ones which have become conventional (which is not to say always unchallengeable) during the last decade. In all, Griffiths has produced an unpretentious little volume with a good deal to be unpretentious about.

H. G. GELBER
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UNITED STATES

DAVID B. QUINN. *North America from Earliest Discovery to First Settlements: The Norse Voyages to 1612*. (New American Nation Series.) New York: Harper and Row. 1977. Pp. xvii, 621. \$15.00.

In the preface to this book David Quinn suggests an approach to the subject and topics that indicates his treatment may be a combination of narrative, analysis, and infusion of ideas and methodologies from the social and behavioral sciences. On the last point, however, the text provides more expectation than fulfillment, for the book contains little that will be unfamiliar to readers of Quinn's numerous writings. In many ways this volume is an admirable digest of the ideas and data to be found in its predecessors from his pen. The post-Columbian history of North America to the time when European settlements were established on the Atlantic seaboard is presented in what Quinn states is a "reasonably balanced view." The substantive and methodological issues that are balanced, and their proportions, are left to the reader's discernment instead of being explicitly stated by the author.

The first chapter explains the Amerindian context which Europeans penetrated. Here Quinn introduces items such as cultural diffusion, processes of cultural exchange, and cultural definition of resources, concepts and models long familiar to the social and behavioral sciences but generally absent from historians' narratives or analysis of early European movement in the New World. But these items are not used as the conceptual framework for the remainder of the book. Instead the

reader is treated to a retelling of de Soto's deprivations, of the ineptitude of most early English settlers, or of exploitations of fish and furs without analyses of these activities as part of the processes of cultural change that was being wrought in North America. The indigenous context is based on a few standard references and lacks the input of Quinn's personal research that is the hallmark of the bulk of the book.

The meaning of Quinn's claim to a balanced view of the period becomes clearer in the remaining chapters. This is not a book solely about Virginia and Québec. In a basic chronological organization, the areas in North America where Europeans were active become the focus of individual chapters. Hence Florida, the interior and littoral of the Southwest, and Newfoundland are treated in detail equal to that traditionally accorded Virginia or Québec. The author relies heavily on his own research in these chapters, and they provide a guide to his prodigious contributions to the study of presettlement Old World-New World contacts. Quinn's ability to incorporate factual data and still to keep the flow of his narrative is commendable and a tribute to his talent as a writer. His reputation as one of the most geographical of historians is enhanced again. Locations, topography, and landscape characteristics share prominence with personalities and politics. Because of his geographical orientation, it is unfortunate that many maps are substandard, particularly those lacking coordinates. Furthermore, he ignores most of the work on the periods by historical geographers, with the exception of Carl Sauer. If Quinn's account is weak on the cultural perspective, it nevertheless maintains an economic focus. And it is refreshing to read a historical economic treatise that avoids dogmatic polemics.

Quinn in conclusion returns to his introductory duality—the American and the European viewpoints on North America's initial exploration and settlement. Although he writes from the perspective of the latter, he acknowledges the need for the former and for a synthesis of the two. He rather too humbly describes his contribution as a general introduction; the book is that and much more. He has demonstrated the virtues and limitations of one approach to the available source materials, and by implication he highlights the need for studies with different methodologies.

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G. E. KIDDER SMITH. *A Pictorial History of Architecture in America*. In two volumes. New York: American Heritage Publishing Company; distributed by W. W. Norton and Company. 1976. Pp. 413; 820. \$45.00.

National Trust for Historic Preservation. *America's Forgotten Architecture*. New York: Pantheon Books. 1976. Pp. 311. \$20.00.

JOHN W. REPS. *Cities on Stone: Nineteenth Century Lithograph Images of the Urban West*. Fort Worth: Amon Carter Museum. 1976. Pp. 99. Cloth \$14.95, paper \$9.95.

All three of these books take a panoramic view of aspects of American buildings and the built environment. G. E. Kidder-Smith, an eminent architectural photographer, traveled the entire country to compile a two-volume pictorial survey which his editors "believe to be the most comprehensive pictorial review of the American architectural scene . . . that has yet been attempted." Brief introductions head each of the regional sections into which the book is divided, augmented by extensive captions with the photographs. But the text is clearly less important than the exceptionally handsome images, too many of which are unfortunately a bit murky in reproduction. The allocation of but one or two photographs per building (with few exceptions), and these without plans, underscores the browsing intent of the volumes. Although their regional organization is convenient for the would-be traveler and faithful to the travels that brought the volumes into being, most readers (or lookers), especially among historians, would find a chronological arrangement more informative.

A similar bicentennial expansiveness of treatment and euphoria of tone characterizes *America's Forgotten Architecture*. In this instance, the preface explicitly mentions the national celebration as the occasion for the publication, sponsored by the National Trust for Historic Preservation. The volume provides a layman's introduction, part explication, part exhortation, to all aspects of the preservation movement. Coverage ranges from superficial discussion of esthetic criteria in evaluating architecture and clues for recognizing styles, to a convenient summary and appraisal of the gamut of present approaches to preservation and the spate of recent legislation making reuse of old buildings feasible. Again photographs generously spice the text; these are less consistently excellent than Kidder-Smith's, but valuable, especially as they augment his views of anonymous buildings and (in line with a major concern of preservationists) feature buildings in their architectural context.

Taken together, the images in these books nicely complement one another to provide as lavish and well-chosen a compendium of American buildings as can be found in any three volumes. For the professional historian the specific contents of *America's Forgotten Architecture*, though helpful, may be of less interest ultimately than the fact that the volume represents a comprehensive bicenten-

nial statement directed to the layman by the National Trust. As such, it becomes a cardinal document on the state and philosophy of the burgeoning movement for architectural preservation.

Finally, John W. Reps' succinct but extensively documented introductory essay on nineteenth-century lithographs of panoramic views of the western city, for the catalogue of an exhibition organized at the Amon Carter Museum of Western Art, distills one facet of a lifetime of scholarly inquiry into the history of images of American urban planning. From the relative anonymity of this kind of imagery he manages to resurrect a considerable amount of information on artists and publishers responsible for the views, on artists' working procedures and techniques, and on publishers' economics and distribution. He comments on the degree to which accurate detail occasionally gave way to boosterism in a situation where "the region was young and every town could dream of a glorious future." I only wish that he had scrutinized these views more intently to ascertain typical ingredients of the western town and their distribution on the land. Did they differ in any respect from eastern towns of the period, or from earlier eastern towns at a similar stage of evolution? Do the images of frontier towns, which came into being with the railroad and the developed factory system as well-established components, and which followed the grid-iron format for street layout that routinely latticed the plains or squeezed and warped mining towns into mountain valleys, suggest any such thing as a *western* town? Or are they merely particular versions of the *American* town inpartially spread from coast to coast? In any event, *Cities in Stone* offers a visual feast of fifty-one views, all in full color.

WILLIAM H. JORDY
Brown University

GRANT GILMORE. *The Ages of American Law*. (Storrs Lectures on Jurisprudence.) New Haven: Yale University Press. 1977. Pp. x, 154. \$10.00.

American legal history is currently undergoing a great academic revival. It is therefore fitting that Grant Gilmore, one of the country's most distinguished legal historians, was chosen to deliver a recent edition of Yale Law School's Storrs Lectures. Inaugurated in 1889, the Storrs Lectures have elicited some of the most profound thoughts of legal luminaries; among the more prominent Storrs Lecturers have been Frederick Pollock, Robert Hutchins, Roscoe Pound, and Benjamin Cardozo. To underline further the renewed importance of American legal history, Gilmore chose as his topic for the Storrs Lectures a review of Amer-

ica's legal past. These lectures have now been published in an expanded and slightly altered form, titled *The Ages of American Law*.

Gilmore's conceptualization of the so-called three ages of American law essentially follows the model proposed by Karl Llewellyn some years ago. The period from about 1800 to the Civil War was the high point of American law. This "age of discovery" saw great legal minds, notably John Marshall, Joseph Story, and Lemuel Shaw, fashion a distinctly American jurisprudence. Their retooling of the federal system and their tinkering with the economy draw Gilmore's highest praise. This golden age was only tarnished by the judicial failure to come to terms with slavery. Following the Civil War came what Gilmore refers to as the "age of faith." Continuing until about World War I, this middle period found lawyers, judges, and law professors expressing absolute confidence in their ability to find truth and righteousness through the law. For Gilmore, the dominant personages of "law's black night" were Christopher Columbus Langdell and Oliver Wendell Holmes. The former gave the new jurisprudence its formalized protoscientific methodology; the latter gave it its harsh content. The twentieth century, however, has witnessed a complete reversal, fostering an "age of anxiety"—a period in which the best legal figures have been naysayers. Gilmore identifies two in particular: Benjamin Cardozo, the skeptical writer and judge, and Karl Llewellyn, the foremost of the "legal realists." They punctured the easy faith of the Langdellians but provided, according to Gilmore, no more than a few unstable trial balloons.

Gilmore's book is a short one and his comments are suggestive rather than probative. Any legal specialist reading *The Ages of American Law* will find much with which to disagree. For example, this reader found the author's treatment of Holmes truncated and unpersuasive and the view of legal realism much too hostile. Yet, Gilmore's penchant for the idiosyncratic and his willingness to challenge widely held generalizations give this book a special character. Throughout, the book is lucidly written and possesses a disarming combination of wit and erudition. *The Ages of American Law* is a notable addition to the legal literature spawned by the Storrs Foundation.

JOHN W. JOHNSON
Clemson University

SIDNEY E. MEAD. *The Old Religion in the Brave New World: Reflections on the Relation between Christendom and the Republic*. (Jefferson Memorial Lectures.) Berkeley and Los Angeles: University of California Press. 1977. Pp. xii, 189. \$10.00.

"This here town just ain't big enough for both of us." Sidney E. Mead in these Jefferson Memorial Lectures throws that folkloric line against the big screen of American religiosity. In his mind there is not enough room in America, or in the minds of any thoughtful citizens, for both the "religion of the Republic" or the Enlightenment and the religion of the sects or denominations. The author thus carries further the themes he earlier developed in *The Lively Experiment* and *The Nation with the Soul of the Church* in his effort to help fellow citizens face up to a need for decision between the two.

As Mead develops the argument in his typically elegant and provocative essays, he sees that Americans who cling to the creeds and organizations of the particular churches have to live with bifurcated minds. The faith they profess in the sects is logically incompatible with the privileged religion of the Republic, the one held by the founding fathers and, thanks to them, enshrined in national rhetoric and institutions.

Mead is certainly correct when he points to the differences between the two. Enlightenment religion demands faith in a Supreme Being and centers in a Jeffersonian "Unitarianism of the First Person" of God, while the denominations tend to grow specific about Trinitarian faith, or about seeing the Supreme Being as the father of Abraham, Isaac, and Jacob, or allowing for pantheism. Mead's description of the faith of the founders is accurate, as is his accounting of the way sectarians in the nineteenth century overwhelmed it, calling it "infidelity."

Here, as elsewhere, Mead does more justice to Enlightenment faith than to the frequent assertions by Jefferson and others that no religion should be privileged over nonreligion or other religions. The novel feature of these lectures appears in the last one where, after denouncing Robert Baird and Horace Bushnell for their particularism and their prejudice against the "infidels," the author lauds Lyman Beecher for remaining both an orthodox evangelical and a celebrator on his own terms of Republican religion. Beecher offers evidence that American creedalists in the churches could work out syntheses in order to overcome intellectual or spiritual schizophrenia. If Mead would pursue other evangelicals down the trail from Lyman Beecher to President Jimmy Carter, and add to their company numerous Roman Catholics and Jews, he would find that Americans have often functioned effectively while granting their due to both of the partly incompatible faiths. Meanwhile, no one has pointed better than he to the tensions between the two, or celebrated more vigorously than he the claim that the Enlightenment religion did and does express the truth about

ultimate reality and provides integration for American life.

MARTIN E. MARTY
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THOMAS C. COCHRAN. *200 Years of American Business*. New York: Basic Books. 1977. Pp. xvi, 288. \$13.95.

Thomas Cochran has written a much-needed book—a balanced synthesis of the history of American business since the beginning of the Republic. No other study has a comparable chronological and topical coverage. The book has many strong points. Its sections on developments in finance, real estate, and business services are most informative, particularly those in the fourth part entitled "The Age of Demand, 1920–1976." Cochran has always been very much at home with the small businessmen and enterprises that populate these sectors of the economy. He does not, however, neglect big business, particularly the large manufacturing enterprise. Here his special concern is for management, and he effectively brings together in brief compass the central points of recent writing on the process and substance of management.

On the whole, Cochran sees the most significant period of business change coming before 1840, not after. In his view, "the business revolution" occurred in the late eighteenth and early nineteenth centuries. It was a precursor, indeed a precondition, of the industrial revolution that followed. Yet on a careful inspection, this earlier revolution may be found to have consisted of little more than a growing use of corporations as a legal form of business, an increased application of existing business methods, such as Italian double-entry book-keeping, and a specialization resulting from the expansion of the nation in area and population. Before the 1840s, moreover, the corporation was used primarily for service enterprises such as banks, insurance companies, and canals. Very few manufacturers and shippers and almost no merchants incorporated their enterprises before the 1840s.

In his introduction Cochran rightly emphasizes that an awareness of the evolution of marketing is central to an understanding of American business history. He does not develop this theme, however, and he underestimates the impact of the revolution in transportation and communication on the processes of production as well as those of marketing and distribution. As long as Americans relied on traditional sources of energy for transportation, communication and production, they were not challenged to develop new business forms, procedures, and institutions. Yet, within a generation

after the widespread adoption of steam power in transportation, electricity in communication, and coal in production, that is, from the 1850s to the 1880s, changes far more fundamental than those that took place before the 1840s had occurred in both the distribution and production of goods. In distribution the merchant had been replaced by the modern forms of marketing institutions: the commodity dealer who bought directly from the farmer and sold directly to the processor; the full-line, full-service wholesaler who did the same for manufactured goods; and the three major types of mass retailers—department store, mail-order house, and chain store. The same years witnessed rapid growth of new forms of mass production, all using coal as their source of energy: continuous and large-batch processes in refining and distilling, metal making, and some mechanical industries; and the fabricating and assembling of interchangeable parts in the machinery industries. The integration in the 1880s of these mass-marketing and mass-production processes within single business enterprises marked the birth of today's giant industrial corporation.

Cochran underplays these basic changes. He devotes but a page to the telegraph and telephone, and portrays the railroads' major role as the pioneering of management techniques. References to the new mass marketers are brief and are described in the section covering the years 1890 to 1930. The new processes of mass production get even less attention. And the integrated enterprise receives short shrift indeed. All this is hardly Cochran's fault. His study, like most such syntheses, is based largely on secondary works. And the current literature of business and economic history reveals relatively little about the impact of rapidly changing technologies and markets on American business practices and institutions. Furthermore, the new speed and volume so fundamental to the revolution in production and distribution had much less effect on finance, real estate, and other service operations, to which Cochran devotes ample space.

No one knows the existing literature of American business history more thoroughly than Cochran, and his synthesis will long remain the standard introduction to the field. Readers will find it a mine of information on the operation of the American economy during the past two centuries, presented with the grace and wisdom that have always characterized Cochran's work.

ALFRED D. CHANDLER, JR.
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BARBARA MAYER WERTHEIMER. *We Were There: The Story of Working Women in America*. New York: Pan-

theon Books. 1977. Pp. xx, 427. Cloth \$15.95, paper \$6.95.

Barbara M. Wertheimer's *We Were There* is the first full-length study of American working women to emerge from the new scholarship in women's labor history. It is a valuable and readable survey of American women's experiences as workers from the earliest settlements of the seventeenth century through the first decades of the twentieth.

Although working conditions are discussed in detail, the book's major emphasis is on women's struggles to unionize. Its narrative of women's efforts to channel their discontent into stable and effective trade unions spans the period from the "turnouts" in the New England textile mills in the 1830s and 1840s through the emergence of national unions and labor federations in the late nineteenth and early twentieth centuries. Throughout her work, the author integrates women's efforts to unionize with the development of a national labor movement. Her detailed descriptions of the countless spontaneous strikes and the slow process of building small women's unions during the late nineteenth century are particularly valuable. Throughout her book, too, the author is sensitive to the unique experiences and problems of black women in the work force, both as slaves and as free laborers. Wertheimer's narrative is enriched by her biographical vignettes of the leaders who emerged among working women, from Salome Lincoln, the young Massachusetts woman who, in 1829, led one of the first women's strikes, to Pauline Newman, one of the first women organizers for the International Ladies' Garment Workers' Union. The author forcefully makes the points that women have been important in the work force throughout American history and that far from being passive, many women were militant and aggressive in their attempts to improve their working conditions. She also discusses the barriers to women's equal opportunity, both in the work force and in the labor movement.

Wertheimer's stress on women's efforts to unionize is both a strength and a weakness. On the one hand, she effectively documents the immense vitality of women's organizing efforts—a story left untold in conventional labor histories. On the other, the reader tends to lose sight of the fact that throughout American labor history, only a small percentage of women have organized successfully. Then, too, because the book is organized primarily around unionization efforts in various trades, the coverage of nonindustrial workers is brief. In particular, the situation of nineteenth-century domestic servants might have been discussed more fully.

Inevitably, a survey such as *We Were There* stim-

ulates questions and suggests new areas for investigation. One of the most important topics that the author touches upon throughout the narrative but does not analyze in detail is occupational segregation by sex. We need to know much more about the origins and effects of sex segregation in the work force. A second area that I wish Wertheimer had analyzed systematically was the relationship between men and women workers on the local level. The support of local union men frequently emerges in *We Were There* as a critical variable in determining women's success or failure in a strike or in building a union. We need to know more about the factors that explain male support or opposition to women's organizing efforts in individual communities and industries.

NANCY SCHROM DYE
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WINTON U. SOLBERG. *Redeem the Time: The Puritan Sabbath in Early America*. (A Publication of the Center for the Study of the History of Liberty in America.) Cambridge, Mass.: Harvard University Press. 1977. Pp. xii, 406. \$18.50.

Although many books have been written about Puritanism, there is something elusive about the New England experience which leads authors of each new monograph to attempt to provide a clue to the whole phenomenon through one of its parts. In the past, scholars have sought that keystone in such topics as the New England "mind," communal life, economic conflict, or the doctrine of preparation. In *Redeem the Time* Winton U. Solberg focuses on yet another element of Puritanism, the Sabbath. He argues that his study will lead us not only to a fuller comprehension of the Lord's day, but also to a better understanding of Puritanism and of American culture as a whole.

Solberg begins this lengthy and well-documented account by describing the Sabbath in Biblical and Early Christian times. He argues that the Puritans were influenced by earlier views, but developed their own distinctive pattern of Lord's Day observance. Under the influence of covenant theology and in response to English socioeconomic conditions, they began to observe Sunday with a thoroughness unmatched by Continental Protestant churches. This new Sabbatarianism was one of the distinctive features of English life in the New World. Although Solberg finds that the practice was especially strong in New England, he concludes that in all of the colonies strict observance of the Sabbath influenced attitudes toward work, the family, spiritual and material values, and the role of the state in religion.

Solberg's account has many merits. It is based

upon a wide variety of sources, which are reflected in a rich collection of footnotes and a useful bibliographical essay. It provides a valuable perspective on the Puritan settlements by describing all of the colonies, and not just New England. Additionally, the book benefits from Solberg's acquaintance with the broad sweep of Christian history.

But unfortunately the conceptual framework of *Redeem the Time* is less commendable than its scope. In the first place, large sections of the book deal with aspects of American history which do not contribute substantially to our understanding of Sabbatarianism. At times Solberg seems to assume that any information about early American religious history is necessarily germane to his subject. This leads to a diffuse quality in some sections of the narrative and detracts from the analytical force of the book.

Furthermore, this tendency to believe that every detail of Puritan life is relevant to an understanding of the Sabbath leads Solberg to exaggerate the importance of the Lord's Day itself, in relation to other areas of religious life. Typically, the settlement of Maine and the Christianization of the Indians appear to have been undertaken primarily to establish the Puritan Sabbath. And Lord's Day observance appears to be the cornerstone of individual piety. Solberg tells us, for example, that in 1613 John Winthrop sought a closer relationship to Christ and so resolved to observe the Lord's Day diligently (p. 69). Apparently Winthrop's Sabbatarianism is the essence of this new religious resolve. But this was only one of many aspects of his religious experience: when we examine Winthrop's own papers, the quoted passage turns out to be number eight in a list of twelve resolves for pious behavior. What is missing here and elsewhere in the book is a thorough analysis of the relationship between Sabbatarianism and other aspects of the religious life.

Moreover, the book leaves us feeling that we have seen the husk, but not the kernel of the Puritan Sabbath. *Redeem the Time* describes numerous laws against improper Lord's Day conduct, but says little about how well-behaved individuals and families actually acted on the Sabbath. The Puritan Lord's Day involved a form of worship which was carried beyond the confines of the church and took place in the home and in the village as a whole. This fact has important implications for the thesis—frequently articulated in the past decade—that Puritanism was merely a religion of the meeting-house. We would profit from knowing more about what happened in those hours away from church. Solberg does claim that the Sabbath experience strengthened the family and reinforced spiritual values, but he fails to support such claims by providing a clear picture of actual Sabbath behav-

ior, resorting instead to clichés such as, it “was an occasion of great joy” (p. 265).

Redeem the Time suffers then from a failure to describe Sabbatarianism fully and to locate it convincingly in a broader context. At times the reader is left wondering whether American culture or even Puritanism itself would have been very different without strict Lord’s day observance. But despite its shortcomings the book is a valuable contribution to our knowledge of an important area of American religion. At the least Solberg has added another piece to the Puritan puzzle by setting out the Sabbath in bold relief as a subject for study.

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FRANK BAKER. *From Wesley to Asbury: Studies in Early American Methodism*. Durham, N.C.: Duke University Press. 1976. Pp. xiv, 223. \$9.75.

Frank Baker’s *From Wesley to Asbury* is a remarkably authoritative, readable, and incisive culmination of years of research into the beginnings of American Methodism. Only one of the eleven essays included in the volume has not previously been delivered publicly or printed elsewhere over a ten-year period from 1962 to 1972. Nevertheless, the uniting and publication of the disparate writings brings together for the first time the pieces of a puzzle which were previously known and appreciated only by a handful of specialists in early American Methodism.

Unlike many students of church history who have written from the provincial perspective of a single denomination, and who may be suspect in terms of accurate scholarship, the British-born Baker, who presently serves as coeditor of the massive project to publish the papers of John Wesley, brought to these papers the meticulous and thoughtful eye of the informed historian. Whether his subject is the Wesleys in Georgia in the 1730s, the career of George Whitefield, the portrayal of such little-known early Methodists as Robert Strawbridge, Barbara Heck, Philip Embury, Robert Williams, and Thomas Webb, or the exploits of the well-known early bishops of Methodism, Francis Asbury and Thomas Coke, his procedure is to sift through the dozens of myths and every shred of evidence to give the most accurate picture possible of the movement of Methodism in the eighteenth century from England to America. In addition to taking a detailed look at the early incidents and people who founded American Methodism, Baker also presents an overview of early American Methodism, arguing convincingly and mindfully that American Methodists were always working within the context, and according to

many of the doctrines and practices, of a movement that had its beginnings in England.

While the book as a whole reveals little that was not previously known to Methodist scholars, and while it does not reveal previously unknown troves of source material, it is perhaps the clearest and most scholarly narrative of the founding and early years of any American religious denomination. Historians will find the volume reliable and useful. General readers will find it interesting and moving.

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PHINIZY SPALDING. *Oglethorpe in America*. Chicago: University of Chicago Press. 1977. Pp. xi, 207. \$12.50.

As an area of English settlement from 1733 to 1743, Georgia, as the quip goes, was too large to be an insane asylum and too small to be a colony. It was, in fact, an American orphanage for imperial “unfortunates” presided over by one of the most enigmatic figures of early American history, James Edward Oglethorpe. Almost every writer who has tried to fathom Oglethorpe’s motives and reasons for undertaking the Georgia adventure has become inextricably entangled in the cloying philanthropy of the Georgia plan itself. Thus, forty years ago Amos A. Ettinger described Oglethorpe as an “imperial idealist” while Daniel Boorstin, writing in 1958, painted him as a “reformist dilettante,” a sort of semihero in an antiheroic age. Now, in *Oglethorpe in America*, Phinizy Spalding presents still another Oglethorpe.

Spalding’s approach is to examine Oglethorpe’s years in America through the prisms of the colony’s first settlements, Oglethorpe’s correspondence and finances, rum and the Indian trade, land grants, Negroes and slavery, and the 1740–42 frontier war with Spain. What emerges is a critical and refreshing picture not only of Oglethorpe but also of the Georgia project itself.

Spalding’s Oglethorpe turns out to be a complex, many-faceted man. He was, for example, a poor correspondent guilty of negligence in not writing the trustees of Georgia’s progress, an agrarian reformer who unrealistically wanted to give everyone a small plot of land regardless of location, and a compassionate, hard-working friend to all the human flotsam and jetsam thrown up on the southern frontier in the 1730s. Spalding also stops just short of labeling Oglethorpe a paranoid, a do-gooder whose hyperactive imagination saw Spanish spies everywhere. As Spalding points out, Oglethorpe blamed every disaster on the east-

ern seaboard, from fires in New York to the Stono Rebellion in South Carolina, on Spanish agents. They were everywhere and their agents had even seduced Carolinians and the Georgia Board in London to turn against him. Moreover, Oglethorpe thought that slaves, "Irish priests, who pass for Physicians, Dancing Masters, Clock Makers, and other such kinds of Rambling Professions" were working for the Spanish. Certainly, such an interpretation does much to explain Oglethorpe's strange, solitary, erratic behavior at times.

The best feature of *Oglethorpe in America* is its readability. Spalding writes in an engaging, elegant style which should be the envy of many in the profession. Strengthened by an excellent bibliography, index, and footnotes, *Oglethorpe in America* should be of particular interest to the discriminating scholar as well as to the interested layman.

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PAUL E. KOPPERMAN. *Braddock at the Monongahela*. Pittsburgh: University of Pittsburgh Press. 1977. Pp. xxvi, 322. \$9.95.

Battles are the stuff of myths, and many myths have grown up around the disastrous defeat of General Edward Braddock on the Monongahela in 1755. In this volume Paul Kopperman sets out to separate fact from myth. It is a careful examination of all known accounts of the battle—including a few which were unknown until he unearthed them. Kopperman does not claim to have produced a new overall interpretation of the battle, but rather one which reconciles discrepancies and contradictions in previous accounts. He has written, although he disclaims that distinction, the definitive narrative of the battle, the account which will stand except in the unlikely event of the discovery of a new, extensive, and highly revisionist first-hand description. The author takes the reader through all the existing accounts (half the book is devoted to a reprinting of them), points out where they are at variance or in conflict, and reveals the reasoning behind the interpretations chosen. Indeed, the book is a masterful demonstration of the techniques of critical analysis and might well be used for that purpose in a graduate seminar.

In the analysis Kopperman largely exonerates Braddock and, insofar as it can be attributed to one person, places the responsibility for the defeat on Lieutenant Colonel Thomas Gage who, as commander of the vanguard, failed to secure the flanks of the army and responded inappropriately when first contact with the French and Indian force was made. But in the end Kopperman agrees with the

conclusion reached by George Bancroft one hundred twenty-five years ago: "... in the final analysis the day was lost, not because one or more officers blundered, but because the regulars panicked. It was they who threw away the great advantage held by the British." The panic arose from "an almost insane fear of the unseen and alien enemy."

The problem with this book is that it is fundamentally antiquarian, an example to be wheeled out by those who wish to demonstrate that history is a process of learning more and more about less and less. Kopperman does not neglect entirely the events leading up to the battle or the consequences of it, but his focus is firmly on the battle itself. For my part, I really do not care whether or not the French surprised the British from trenches. I would have liked very much, though, to have seen an elaboration of the tantalizingly brief last chapter, in which the author deals with the consequences of the defeat, particularly the idea that it destroyed in American minds a myth of an invincible British army and hence paved the way for the Revolutionary War.

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KENNETH S. LYNN. *A Divided People*. (Contributions in American Studies, number 30.) Westport, Conn.: Greenwood Press. 1977. Pp. 113. \$11.95.

In three deft essays on loyalist and patriot backgrounds and the psychological dimensions of the Revolution, Kenneth S. Lynn makes a zestful, arresting contribution to the popular scholarship of the Bicentennial. Characteristically exact about how he came to write the book, Lynn tells of reading a spate of Revolutionary biographies and being appalled by their psychological shallowness; then Edwin G. Burroughs' and Michael Wallace's celebrated essay on "The American Revolution: The Ideology and Psychology of National Liberation," (*Perspectives in American History* [1972]) encouraged him to explore the childhood experiences of thirteen loyalists and eighteen patriots in search of clues to their Revolutionary allegiance.

The strength of Lynn's book is his ability to knit biographical fragments into vignettes and to detect in a series of vignettes a pattern of character development. As an authority on nineteenth-century literary history, Lynn brings to this study a powerful sense of story, prose, and dialogue. He finds in all the loyalists examined some form of dysfunction in their relationships with their fathers—oppressiveness, possessiveness, or neglect—and all but three of the fathers of his patriot subjects were

"restrained" in their exercise of parental authority. Lynn cautiously suggests "the haunting possibility" that the origins of the Revolution lay in the childhood experiences of the Revolutionary generation.

It is a plausible, engaging, modest argument, and those merits obscure its weaknesses. Most serious is Lynn's heavy-handed approach to Revolutionary historiography. He argues that Bernard Bailyn's *Ideological Origins of the American Revolution* fails to explain the decision for independence, and from this premise makes a breathtaking leap to conclude that his own brand of prosopography, carried far enough, will unlock the secrets of the Revolution. Though he acknowledges the "complexity" of this subject, his historiographical categories are forced and arbitrary. Lynn misses an opportunity to examine how individual experience reflected, transformed, and interacted with ideology, tactics, and politics. Basing his work too narrowly on formal biography and autobiography and on the speculative findings of Burroughs and Wallace, Lynn neglects resources like Bailyn's sketch of John Dickinson, Robert M. Weir's articles on South Carolina political culture, Peter Shaw's book on John Adams, and Adrian C. Leiby's on the New Jersey Dutch. These works superficially support Lynn's formula, but at a deeper level they show how ideology, family life, religion, social and personal relations, and political pressures interacted in a complex fashion to shape Revolutionary allegiance and that no single factor—not even parental affection or disfavor—had peculiar strategic leverage.

Nonetheless, Lynn's book, at turns playful and serious, should provoke and delight the undergraduate and general audience for which it is well suited.

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STEPHEN E. LUCAS. *Portents of Rebellion: Rhetoric and Revolution in Philadelphia, 1765-76*. Philadelphia: Temple University Press. 1976. Pp. xxi, 333. \$17.50.

This is a book that could easily be neglected but ought not to be. Because it doesn't seem to say anything startlingly new, it is apt to get lost in the glut of post-Bicentennial books on the Revolution. Yet beneath its conventional retelling of the developing revolutionary movement in Philadelphia from the Stamp Act to the Declaration of Independence, it offers us some tentative first steps out of the historiographical impasse our writing about the Revolution is now in.

Stephen E. Lucas is not only interested in medi-

ating between the presently deadlocked ideological and materialist interpretations of the Revolution; he is also attempting to contribute to the science of human communication in revolutionary situations. His laboratory is pre-Revolutionary Philadelphia, and his subject is the American Whigs' rhetoric. By analyzing the Whigs' ideas as rhetoric, that is, as strategic communication designed to influence particular audiences at particular times and places, Lucas hopes "to explain how the public discourse of Whig writers and speakers contributed to the development of revolutionary beliefs and behaviors in colonial Philadelphia."

Such a manipulative view of rhetoric immediately calls to mind the propaganda studies of the 1930s, which treated ideas as devices shrewdly used by leaders to influence the opinion of a passive populace. But since this conception of ideas as propaganda has been so generally discredited as naive and crude in its relating of ideas to behavior, Lucas is very anxious throughout to distinguish what he is doing from what the propaganda-minded historians did. To acknowledge that the Whigs shifted and tailored their thought to fit changing situations, Lucas argues, "impugns neither the relevance of that thought to the revolutionary movement nor the motives of Whig spokesmen" (p. xvi). Lucas stresses over and over that the Whigs were sincere in their beliefs, and that the populace was not an empty vessel. Despite Lucas' strenuous efforts to distance himself from the propaganda studies, however, many historians will still see his book as an attempt to refurbish the older progressive approach to the ideology of the Revolution.

Lucas ought not to be at such pains to hide or deny this, for such a refurbishing has been long overdue. We don't have to return to the crudities of the propaganda studies to realize that men attempt to manipulate language and ideas in order to persuade, lay blame, and legitimate behavior. If those historians who deal with ideas continue to insist that such ideas are important only as "sincere" expressions of motives for action, then they and the ideas they work with will never be taken seriously by their more hardheaded colleagues who know better how people really behave. The questions we have to ask of ideas in a historical situation are not whether they were the causes, that is, the sincerely held motives, for action, or whether they were the covering consequence of some hidden interest, or whether they accurately depicted reality. Instead, we have to ask what did the ideas do in that historical situation and why did the historical participants use particular ideas in the way they did. The Whigs' belief in certain constitutional principles or even their belief in a British conspiracy against American liberty did

not "cause" the Revolution in the sense that such beliefs were the springs for Whig behavior. Such a causal explanation in such exclusively intellectual terms will never be fully convincing.

Lucas is aware of some of these problems, but he has scarcely sorted them out with any clarity. He remains convinced that ideas can be treated as motives for action and seeks to measure the social influence of the Whigs' ideas even though he repeatedly admits this is impossible. Whether his book has contributed anything to the science of communication seems doubtful, and it surely has added little to our knowledge of the coming of the Revolution in Philadelphia. Nevertheless, by examining ideas in a functional manner as rhetoric, Lucas has opened up the question of the intellectual origins of the Revolution in a refreshingly new way.

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JOHN S. PANCAKE. 1777: *The Year of the Hangman*. University: University of Alabama Press. 1977. Pp. viii, 268. \$11.95.

John S. Pancake, the latest writer to survey the events of 1777 during the American War of Independence, has composed a lively, generally well-reasoned narrative of the crucial "year of the hangman" (so named because, as John Adams observed, the date "had three gallows in it"). Not since Hoffman Nickerson's *Turning Point of the Revolution* (1927) has anyone attempted in one volume specifically to analyze John Burgoyne's campaign in upstate New York, and only Pancake has combined with the story of that invasion an account of William Howe's maneuvers in Pennsylvania.

In this small book, Pancake covers a great many subjects. He devotes more than a fourth of his space to preliminary background and includes material on military actions, Lord George Germain's war efforts, assessments of important figures such as the Howe brothers and Lord North, suppression of loyalists by American militia, and British planning for the campaign. The rest of his account is given over to his main subject, a detailed analysis of the events of 1777. Much of this material is well known to scholars, but occasionally Pancake makes a fresh observation. For instance, he notes that the main role of patriot militia in the war was to act as a local police force compelling dissidents to obey the will of the rebels, and he comments favorably on John Shy's astute observation that "the war was a political education conducted by military means."

Pancake's research is on the whole careful and sound; he makes good use of printed primary ma-

terials and judiciously blends in secondary works. He depends too heavily, however, on a few sources which scholars generally consider untrustworthy, such as James Wilkinson's *Memoirs* and Thomas Anburey's *Travels*. Other sources that he omitted but might have studied with profit are Richard J. Hargrove's "General John Burgoyne, 1722-1777" (Ph.D. dissertation, Duke University, 1971) and my articles on Horatio Gates' activities in the campaign of 1777.

One of the author's conclusions about the campaign of 1777 is interesting but questionable. He asserts that Gates and Israel Putnam ruined Washington's plans in late 1777 by refusing to detach any of their troops during or after the conclusion of the Saratoga campaign, thereby thwarting the commander-in-chief's intention to force the surrender of William Howe in Philadelphia before Admiral, Lord Howe's fleet could open the Delaware River and bring supplies to the British army. Pancake labels the unwillingness of Gates to part with soldiers in mid-campaign as "incipient insubordination," but he weakens his own argument by pointing out that Washington at first only "asked" Gates for assistance (p. 197). Moreover, he ignores Bernhard Knollenberg's point, in *Washington and the Revolution, a Reappraisal* (1940), that Gates and Putnam did not hesitate after Burgoyne's surrender to send most of their Continental regulars to the commander-in-chief.

All in all, Pancake's book is a timely addition to the literature of the War of Independence, useful both to scholars and to general readers.

PAUL DAVID NELSON
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JOSEPH L. DAVIS. *Sectionalism in American Politics, 1774-1787*. Madison: University of Wisconsin Press. 1977. Pp. x, 239. \$17.50.

Sectionalism has intermittently obtruded itself at crucial moments into national affairs in our past, but scholars have usually associated it with post-1789 events. Joseph L. Davis has chosen to examine the Revolutionary era in order to pinpoint the initial emergence of regional consciousness. More specifically, this author intends "to show how sectionalism, in all its ramifications, complicated the struggle between the proponents and opponents of stronger central government . . ." His rationale for this study is that historians have too commonly dismissed sectionalism as a significant force before 1789.

Although the problem is a major one, the writer has chosen a very narrow focus for his investigation. Much of the narrative revolves around congressional actions with some references to lead-

ership opinion in a number of the states. Within this context Davis examines the role of sectional pressures in a series of key issues: the impost proposal, Spain and the navigation of the Mississippi River, and the movement to revamp the Articles of Confederation, among others. Treatment of the war years requires only 58 pages whereas the last four years, 1784–87, encompass 128 pages.

Unfortunately, the conceptualization ignores the necessity for a definition of sectionalism. The reader finds an implicit assumption that the states north of Maryland comprised the North, whereas those south of the Mason-Dixon line were the South. Furthermore, one learns that the North subdivided from time to time into the New England and Middle states. It is true that historians have long employed these categorizations, but any use of them to explain the influence of sectionalism must begin by analyzing those interests, institutions, and cultural and behavior patterns which these states possessed in common. Finally, sectionalism may be intrastate as well as interstate. The clashes within states between backcountry and commercial areas constituted an integral portion of the evolution of many of the United States. Yet Davis only adverts to these fundamental considerations in piecemeal fashion, when he discusses certain specific issues.

On another conceptual level there is an obstacle of similar magnitude: on what basis can historians select some leaders' opinions as representative of their sections? Davis confronted the difficulty and resolved it in unsatisfactory fashion. "When a specific stance is identified as southern, northern, or New England no implication is intended that the interest of some merchants or some planters should stand for entire regions or classes" (p. 4n). Nonetheless, the labeling of the views of a Madison or Higginson as those of their sections is almost inevitable. If these men did not speak for their sections, for whom did they speak? Did they speak only for a section of their class in their state? If they reflected the opinions of a fraction of their class, they cannot be said to have represented sectional opinion.

In general the author's research suggests the incipient state of sectional feeling. More often than not in these years the unity of groups of states on the most acrimonious issues tended to be unstable, temporary, and relatively easily broken. Perhaps the most striking symptom of the newly emerging regionalism was the heated debate over the negotiation of a trade treaty with Spain which would have suspended for twenty years U.S. passage down the Mississippi. The southern states voted solidly against closure of the river, partly, the author contends, because they feared a diminution of their power in the Confederation.

Even if one accepts Davis' central assumptions about sectionalism, the evidence produced here does not support the conclusion that regionalism was a major element in the evolving conflict over the viability of the Articles of Confederation. Rather the events described lend themselves to the generalization that the states lacked strong sectional bonds and that by 1787 other interests and influences shaped the drift to Philadelphia.

BERNARD MASON

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ABRAHAM D. SOFAER. *War, Foreign Affairs and Constitutional Power: The Origins*. Cambridge, Mass.: Ballinger Publishing Company. 1976. Pp. xxxvi, 533. \$15.00.

In 1971, at the height of the constitutional debate that finally produced the War Powers Act, the American Bar Association commissioned a study "on the respective powers under the Constitution of the President and of Congress to enter into and conduct war." The result is this volume by Columbia law professor Abraham D. Sofaer covering the writing of the Constitution, and executive-congressional relations during the first six presidential administrations (through 1828). Two succeeding volumes are planned to bring the study to the present.

The value of this book is its objective tone and its thoroughness. Sofaer rightfully insists that a discussion of war powers must include such questions as presidential control of information, the use of military force in situations short of declared war, congressional appropriations, and control of foreign affairs generally. The book is soundly and carefully reasoned. And it concludes quite rightly that while the Constitution invests Congress with powers sufficient to determine and control war, Congress began in the Washington administration to delegate to the executive such vast discretion over foreign relations and such latitude over the use of military forces that the president, who in any case possessed overlapping authority, became virtually the arbiter of war and peace. Thus from the beginning, the "imperial presidency" lay inherent in constitutional practice (if not necessarily in the Constitution itself or in the intent of the framers).

Unfortunately, the flaws in this volume far outweigh its contributions. In spite of numerous references to politics, partisan activity is not integrated into the narrative, and the chart of party lineups contains numerous errors. Had Sofaer studied the nationalists of the 1780s, and consulted the standard works on them, he might not have concluded

that the Constitution apparently "grants to Congress overwhelming authority to control all military decisions other than tactical" (p. 38). The nationalists in fact meant to give the executive broad power to conduct war once Congress decided on it; hence the convention gave Congress power to "declare," not "make," war. Moreover, sixty pages on the origin and writing of the Constitution are not adequate to provide anything more than a superficial survey. Most important of all, the book lacks any explanation of the events it describes. Why did presidents quickly begin to pre-empt the war powers; why did Congress so willingly forfeit them? The research concentrates too heavily on the executive instead of Congress, and too much on what happened instead of why. Without interpretation, we are left with a dull, bloodless recitation of facts, and a summation of trends, most of which have been long known to historians. Undoubtedly after the confusion surrounding the Vietnam war, such a catalogue is useful, but it constitutes an opportunity lost for learning the intentions of the framers of the Constitution, whether those intentions have been carried out since 1789, and if not, why not.

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STEVEN J. NOVAK. *The Rights of Youth: American Colleges and Student Revolt, 1798-1815*. Cambridge, Mass.: Harvard University Press. 1977. Pp. vii, 218. \$10.00.

The central argument of this book is that between 1798 and 1808 a nascent student movement arose, bringing rebellion and reaction to American colleges. The movement was the product of a post-revolutionary generation that "needed a cause which would prove their manhood and establish their identity" (p. 42). Steven Novak sees students struggling like revolutionaries for their rights but inadvertently generating a reaction that would clamp down harsh discipline and cause the colleges to decline, enrollments to contract, and the Enlightenment to fade.

Novak's sources are chiefly "contemporary opinion as revealed in letters and pamphlets," and his method aims to place "student revolt in the center of turn-of-the-century intellectual history." He has found literary sources, both published and in manuscript, particularly for narratives of rebellions and revivals at Princeton, Harvard, North Carolina, William and Mary, Virginia, Yale, and Dartmouth.

The book is written in dissent from recent studies of the social origins of students and the social analysis of group violence. Novak invokes the au-

thority of John Shy to dismiss the need to deal with riots in a quantitative manner, though he offers a reading of Phyllis Vine's quantitative dissertation (directed by Shy) to suggest that there were no social changes in the student population during his time span. He offers instead a narrative of selected rebellions at selected colleges, using literary sources. The rebellions are not discussed systematically. Novak does not define the social composition of rebels or their leaders, or what a rebellion was. Since he declines to incorporate a minimal statistical awareness, Novak resorts to language of guesswork whenever he confronts a problem of number, scale, or frequency: "often," "might," "most," "many," "sometimes" (pp. 130-31).

Novak wants to refute recent revisions in the historiography of colleges and student populations, both in his method and thesis. The major problem is that his book does not deal consistently with students themselves; it relies heavily on adult opinion about students. Methodologically, the book makes no advances. In its thesis, it revives the work of Richard Hofstadter, seeing a decline in the old-time college after 1790, a decline Novak attributes to student rebellions. In doing this, he has employed admirable research in literary and archival sources. And he has contributed by drawing our attention to a possible political awareness among students, especially after the crisis of 1798, though he says, curiously, "Student petitions and manifestoes contained virtually no ideology" (p. 77). The book does take significant events seriously, but there is more work to be done using other approaches to the same events.

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CHARLES M. WILTSE, editor. *The Papers of Daniel Webster: Correspondence*. Volume 1, 1798-1824; volume 2, 1825-1829. Hanover, N.H.: University Press of New England, for Dartmouth College. 1975; 1976. Pp. xxv, 518; xxiv, 563. \$20.00; \$20.00.

In 1971 the definitive collection of all extant papers of Daniel Webster was made available on microfilm. Now the editors of the *Papers of Daniel Webster* have inaugurated the letterpress collection of Webster's correspondence, legal and diplomatic papers, and formal writings and speeches. The series, which will eventually run to fourteen volumes, is designed for the "general reader and . . . the student of the period rather than the man." These two volumes, comprising skillfully selected portions of Webster's correspondence from 1798 to 1829, constitute an auspicious beginning of the published Webster *Papers*.

The craftsmanship of the editing merits first note. Introductory sections and compact editorial

paragraphs lucidly set the context for the letters and provide a succinct narrative account of these thirty years of Webster's life. The final pages in each volume provide a complete calendar of the microfilmed Webster correspondence, with a one-sentence description of most letters and a citation to the appropriate microfilm frame number. As a visual bonus, forty-six pages of illustrations in the two volumes provide vivid portraits of the *dramatis personae* of the period; those of Webster himself powerfully convey the dramatic physical presence that so awed his admirers.

For the student of nineteenth-century America, the Webster letters offer insight into the social history of the period as well as the political metamorphosis of the nation. Webster was among the most able of the early Republic's rural young men of small means who found both the professions and the politics of the new nation open to talent. He was the first in his farming family to be schooled for college; luck, audacity, and a well-placed recommendation won the Dartmouth graduate a clerkship in the law office of a leading Boston Federalist. Confirmed in his talent, exhilarated by the cosmopolitanism of the city, encouraged in his ambition, Webster concluded that "cash" rather than social background constituted the "real, unavoidable aristocracy that exists and must exist in Society." When he moved to the busy port city of Portsmouth, his ability soon won him rank at the bar and entry to the best society. Upward mobility gave him much to conserve.

The first volume of the correspondence suggests the crucial importance of Webster's legal career to his development as a political leader. The letters show the care he took as an attorney and the ever rising prominence of his clients. As Webster rode circuit with able Federalist attorneys, he honed his eloquence and sought fundamental and settled principles as the basis for his arguments. Elected to Congress in 1813, the gifted young lawyer made the most of defending his minority party and section, and in 1814 he privately endorsed the Hartford Convention resolutions as a "moderate" recourse to Mr. Madison's war. Webster left national politics and moved to Boston in 1817. Over the next half-dozen years, in a burst of accomplishment, he won climactic cases before the Supreme Court and awed Boston with displays of oratorical skill. When he returned to the House in 1823, it was—in the words of an observer who found him personally "proud and haughty"—as a "Sampson" among "pigmies."

The remainder of the Webster letters remind us just how kaleidoscopic and shifting the political realignment of the 1820s was to contemporaries. Federalism as a political party was dead, but animosity to former Federalists persisted. Webster thought that the cause of internal improvements

would make past quarrels obsolescent and end New England's isolation. Endorsing Adams for president, embracing Clay's "American System," he became a member of the emerging National Republican coalition, only to see congressional supporters of Andrew Jackson fashion a powerful opposition organized around hostility to Washington activism and suspicion of the president from New England. Surprised by the success of this negative coalition, Webster and other National Republican leaders were caught short again by the organized network of opposition newspapers that emerged full blown in 1827. The letters show that Webster played an important role in trying to mobilize the National Republicans, by securing funds to underwrite their own newspaper network and by pressuring the president to employ patronage more astutely.

Of Webster the thinker these letters reveal little; of Webster's personal life the correspondence offers glimpses. By and large the letters tend to the immediate concerns of politics, law, and business. Clues to the metamorphosis of his thought, for this period at least, must largely be found in the drafts and final versions of his speeches, pamphlets, and legal briefs. Better than any earlier collection of Webster letters, the *Papers* provide a view of Webster's family life and particularly of his wife Grace. As Webster rose in politics, his wife retreated into domesticity. When he returned to Washington in 1824, she remained in Boston, confessing that she was mortified "to reflect how much I am behind you in everything," worrying about being despised because inferior.

Nuggets of new material embellish the volumes, but the real achievement of the published correspondence lies elsewhere. The letters allow the reader vividly to witness the growth of one of the nineteenth century's leading figures, and provide a coherent view of a man whose unfolding political, professional, and personal life reveals much about the expansive young republic.

SYDNEY NATHANS
Duke University

VINCENT PONKO, JR. *Ships, Seas, and Scientists: U.S. Naval Exploration and Discovery in the Nineteenth Century*. Annapolis: Naval Institute Press. 1974. Pp. xii, 283.

"At no period since the days of Columbus and Cortez has the thirst for exploration been more active and universal than now," the *Washington National Intelligencer* told its readers on Columbus Day, 1853. Those who extended the frontiers of civilization on behalf of pre-Civil-War America were mainly members of the armed forces. The role of the army in the opening of the American West is a familiar chapter of our history. The

equally significant role of the navy, which carried the same spirit of adventure to the Near and Far East, to West Africa, to South America, and above all to Antarctica and the South Pacific, has not received adequate scholarly treatment.

Vincent Ponko's aims are modest in his account, based on manuscript and printed primary sources, of the navy's expeditions to foreign lands. Rather than "a synthetic history of U.S. naval exploration before the Civil War," he offers "a worthwhile condensation, arranged for reading, as well as a take-off point for additional inquiry" into "specific expeditions that have received extensive coverage elsewhere."

Of the dozen expeditions whose details Ponko narrates, only the Wilkes Expedition antedates the Mexican War. That, like Lieutenant William F. Lynch's reconnaissance of the Drift Sea and the River Jordan (1847-49), aimed at increasing understanding of hitherto little-known regions of the earth. The new nation, America, strove to match the contributions to geographical knowledge of its parent nations in Europe. The two expeditions to the Arctic (1850-55) formed the American contribution to the extensive search for the lost Sir John Franklin.

Despite Ponko's title, only these four and one other expedition may be called scientific: the other is the Astronomical Expedition to the Southern Hemisphere (1849-52), the only expedition devoted to pure science. The remaining expeditions, all in the 1850s, were not scientific but imperialist. M. C. Perry in Japan and T. Page in Argentina tried to prove the old dictum that trade follows the flag. The Ringgold-Rodgers Expedition charted the oceans where American whalers found their best catches. Naval parties explored possible canal routes in what is now Panama. Lynch went to West Africa to determine whether colonies of freed slaves might be established elsewhere than in Liberia. And W. L. Herndon and L. Gibbon explored the Amazon Basin, where slavery might flourish if it were driven from the American South. Behind most of these thrusts lay the unflagging energy of Matthew Fontaine Maury, no scientist (in spite of his claims) but a Southern imperialist who knew how to harness the navy to the yoke of Manifest Destiny.

The author has announced that he is preparing a sequel. For it he should bear in mind two things. First, a detailed narrative that avoids interpretation requires exceptional writing skill to sustain the reader's interest. Second, a book on exploration that contains not a single map is hard to follow.

HAROLD L. BURSTYN
U.S. Geological Survey

SEYMOUR V. CONNOR and JIMMY M. SKAGGS. *Broadcloth and Britches: The Santa Fe Trade*. College Station: Texas A & M University Press. 1977. Pp. xii, 225. \$10.95.

At first blush one might question the need for still another book on the Santa Fe Trail and trade. *Broadcloth and Britches* by Seymour Connor and Jimmy Skaggs is, however, a far more comprehensive work than the title implies. Using a mass of primary and secondary materials the authors have put the origins, development, decline, and impact of the trade in a broad and clear historical perspective.

Citizens of the Spanish and later Mexican outpost of Santa Fe, far removed from sources of supply, welcomed the arrival of the first American traders from Missouri in 1821. Reports by these traders of an eager and lucrative market spread throughout the United States and resulted in a thriving trade that lasted for a half-century, and which did not actually end until the arrival of the Atchison, Topeka, and Santa Fe Railroad in 1880. The trade grew and prospered, despite initial ignorance of the best routes, increasing Indian hostility, and the uncertainties of Mexican commercial policies. Texas independence in 1836 along with extravagant boundary claims by the new Texas Republic threatened to disrupt the trade but determined businessmen supported by the American government ward off the threat.

The end of the Mexican War and the acquisition of New Mexico by the United States was a boon to the trade but did not end continuing difficulties. A movement for secession and Confederate invasion were problems of the early 1860s, and serious Indian warfare continued into the 1870s. Americanization of New Mexico caused a change in the nature of the trade and the aforementioned coming of the railroad wrote the final chapter.

Connor and Skaggs have written a scholarly and thoroughly entertaining book. It is a welcome addition to our understanding and appreciation of the history of the Southwest.

WILLIAM H. LECKIE
University of Toledo

EDWARD A. LUKES. *De Witt Colony of Texas*. Austin, Tex.: Jenkins Publishing Company. 1976. Pp. 269. \$12.50.

The De Witt colony was one of at least a dozen colonization enterprises in Texas authorized by the Mexican government during the 1820s. In this book, Lukes looks at the *empresario* system in microcosm, surveying the development of Green De

Witt's colony from its contractual origin in 1825 to the war for Texas independence in 1835–36. He discusses the painful foundings and slow growth of the first six years, the colony's exposed geographical position on the southwest flank of Anglo-Texas, the intermittent Indian raids that discouraged immigration, the platting of Gonzales, the capital (and only) town, colony government under the *ayuntamientos*, and local events that led to the Texas Revolution. Much of what he covers is familiar to readers of the *Quarterly of the Texas State Historical Association* (1897–1912), and where Lukes has made use of archival materials, his revelations are of the antiquarian variety. He does not bring new insights to the data. Nor does he compare or relate the De Witt colony to other *empresario* efforts, or consider it in the context of other new settlements and the process of community building on the American frontier. Where the evidence is scanty, his speculative conclusions seem idle and contradictions may appear in statements of fact, particularly in regard to population estimates.

In setting out and reconciling some of the details of the Mexican system of land distribution and colonial administration, the book is not without merit. As a synthesis, it should be helpful to new students of Texas history and to local county historians. But it is marred by faulty editing and proof-reading. "Interestingly" introduces at least five sentences and "hopefully" a couple more. The reader's eye is arrested on no less than twenty-five pages by typographical errors, misused words, grammatical mistakes, and occasional malapropisms. The maps and graphs are tedious, and in return for the obvious labor, convey very little useful information.

MARK E. NACKMAN
University of Washington

MARTA WEIGLE. *Brothers of Light, Brothers of Blood: The Penitentes of the Southwest*. Albuquerque: University of New Mexico Press. 1976. Pp. xix, 300. \$12.95.

In their eagerness to record the bizarre and the colorful, nineteenth-century romantics seized upon the "Penitentes" with gusto. "Aye, verily," wrote Charles Fletcher Lummis in 1893, "a procession in which voters of this Republic shredded their naked backs with savage whips, staggered beneath huge crosses, and hugged the maddening needles of the cactus; a procession which culminated in the flesh-and-blood crucifixion of an unworthy representative of the Redeemer." If such reports, ignoring completely the beneficial social services performed by the Brotherhood of Our Fa-

ther Jesus, were distorted, no matter—they made sensational copy.

With *Brothers of Light, Brothers of Blood* Marta Weigle corrects the distortion. Moreover, she proves that penetrating scholarship and respect for one's subject are compatible. Her writing is blessedly free of jargon, justification enough for teaching anthropologists (and historians) English. The book's three parts—brief geographical sketch, historical survey, and discussion of Penitente organization and ritual—are supplemented by transcripts of documents and bibliographical essay.

By the early nineteenth century New Mexico's Franciscans were dying off. Few diocesan priests aspired to a life of poverty. For lack of clergy the sick died without viaticum. How resentful must these poor provincials be, thought a Mexico City attorney in 1832. Resourceful would have been a better word. For mutual aid, fellowship, and religious expression village laymen banded together, "one of many responses," says Weigle, "to a variable, long, and difficult frontier situation." What irked their bishop, when he finally visited them in 1833, were the alleged displays of unsupervised corporal penance, an abuse he could do little about from Durango, seven hundred miles away.

So the village brotherhoods persisted, helping to cushion the cultural shock of U.S. occupation. Driven to secrecy by gawking Protestants and an unsympathetic Catholic hierarchy, Penitentes in northern New Mexico and southern Colorado continued, in Weigle's words, "to serve community needs and to enact spiritually beneficial rituals." A small remnant, formally recognized by the Catholic Church in 1947, survives today.

Unruffled by controversy, the author considers each theory purporting to explain the brotherhoods' origin, then opts for a local lay adaptation of New Mexico's long Franciscan tradition. While admitting excesses, she finds absolutely no evidence that nails were ever used in the simulated crucifixions. Even the symbolic seal of obligation cut in the penitent's back had a practical purpose, assuring "a free flow of blood so the scourgings would not cause welts and bruises."

This is beyond all question the best book on the subject. Latter-day Lummises may choose to ignore it, but no one else can.

JOHN L. KESSELL
Albuquerque, New Mexico

ROBERT DAVID THOMAS. *The Man Who Would Be Perfect: John Humphrey Noyes and the Utopian Impulse*. Philadelphia: University of Pennsylvania Press. 1977. Pp. xii, 199. \$12.95.

This carefully researched and insightful volume might well have been called "Young Man Noyes," since two-thirds of the study focuses on the turbulent years between John H. Noyes' twentieth and twenty-seventh birthdays. These were the years when Noyes underwent several spiritual crises, broke with his family and friends and forged his commitment to a Perfectionist community.

Robert David Thomas sees Noyes as a highly complex and conflicted young man who passively embraced Perfectionist theology in an effort to control his aggressive, assertive nature. When Noyes adopted the practice of male continence and used coitus reservatus to keep the passions in check he was, in effect, following the advice later found in William Alcott's *Letters to Young Men*: "Keep cool, keep cool." The strongest sections of the book center on Noyes' relationship with his family, though the source material for psychoanalytical conjectures about father/son patterns are scant. When Thomas discusses Noyes' passionate love for Abigail Merwin he is forced to rely on the two volumes of correspondence edited by G. W. Noyes. Scholars have never had access to manuscript materials in the Oneida Collection at colony headquarters, and until they become available students have to move cautiously—particularly when one is presenting a psychoanalytical theory based on a close reading of the texts.

Noyes sought security throughout his life and sought to control his inner passions and the outward excitements that threatened to plunge him into antinomian excess. His Perfectionist theology, his structured and conservative communal family, and even his sexual theory protected him from those excesses. Thomas argues that Noyes' personal need for perfection, dominance, and control resulted from tensions within antebellum America. The rise of the city, the decline of the family, the shifting status of women—all these changes forced Noyes to erect a social system which contained those destructive factors. Noyes believed himself an innocent in a corrupt world, and his every action was directed at mastering his own fate and the fate of his followers.

It is when Thomas moves from individual ego development to the general cultural configuration that his argument wavers. He makes too little of the powerful revival tradition and too much of the *angst* of the age. There is no doubt that Noyes was a conflicted and sometimes tormented man; yet, how does one explain his active and passionate involvement with New York City where he lived from 1850 until 1854, and his eagerness to adopt the bone-crushing industrial order implicit in trap production? A simple answer may be that he liked New York with its cultural life and contacts—and

could practice free love there, but not in Madison County, New York. And that he adopted the crunch of the beaver trap because it provided industry for a faltering agricultural colony. In short, he liked his sex—however conservative—and the community needed money.

This is not to trivialize Thomas' book, but to remind historians that there are still some simple facts about communal history that escape notice. For example, was Noyes a real "free lover" and a lecher, or was he, as his descendants argue in several books, a man ahead of his time, with progressive ideas and clean fingernails? Perhaps he was both. Thomas has succeeded in rescuing Noyes from his own mythology and the liberal canonization of his career. By using a psychoanalytical approach, we can now see a man of emotions, a man of passion, and a family man.

ROBERT S. FOGARTY
Antioch College

MICHAEL B. DOUGAN. *Confederate Arkansas: The People and Policies of a Frontier State in Wartime*. University: University of Alabama Press. 1976. Pp. viii, 165. \$8.50.

Just over fifty years have elapsed since the David Y. Thomas volume titled *Arkansas in War and Reconstruction* appeared. Since then extensive new research material has come to light and some interpretations have become obsolete. This is the justification for *Confederate Arkansas*. The author does not concern himself with military operations, central to the completeness of any book-length study of the Civil War in Arkansas. He has, however, ably pushed back the frontier of knowledge on significant nonmilitary aspects of the war.

When Arkansas joined the Confederate states on May 6, 1861, the state contained people of four distinct types with little in common: mountaineers, yeoman farmers, large slaveholding planters, and swamp-dwellers, hunters, and poor white trash. Although Arkansas became a state in 1836, it lacked unity of purpose and developed slowly before the Civil War. Political affairs were controlled exclusively by an entrenched oligarchy.

Secession in Arkansas was ramrodded by the political elite, against the wishes of the majority of the state's citizens. The state never wholeheartedly supported the Confederacy, and even before the end of the first year of the war peace societies sprang up in at least a half-dozen of its northern counties. The Confederacy virtually abandoned Arkansas after the defeat at Pea Ridge in 1862. Slaves soon deserted wholesale, and general economic and social chaos prevailed. The war set the state back twenty years economically, did not solve

the problems of accommodating the two races, but broke the rule of the planter class.

The quality of research in the book is excellent and the writing style is superior. An imbalance in emphasis is apparent, however, for four chapters are devoted to the background of the war while only five chapters are used for the war itself. More interpretation would have been desirable at the conclusions of the chapters and at the end of the book. A little additional editing would have provided first names for James Montgomery and William C. Quantrill. Although the endnotes are ample, a bibliography is lacking. Overall, the study is a worthwhile basic research contribution and is most deserving of the Mrs. Simon Baruch University Award, offered by the United Daughters of the Confederacy, that it won.

LEROY H. FISCHER
Oklahoma State University

MELVYN HAMMARBERG. *The Indiana Voter: The Historical Dynamics of Party Allegiance during the 1870s*. Chicago: University of Chicago Press. 1977. Pp. xi, 251. \$17.50.

Indiana's civic culture has long been characterized by high levels of citizen participation. Indeed, it has often seemed that most Hoosiers were born with a basketball in one hand and a ballot in the other. And there may have been times in the past when Hoosiers took their politics and their parties even more seriously than their basketball. Whether or not that was normally the case, the hallmarks of the state's political culture have been high levels of mass electoral participation and intense partisanship. Even the post-1896 turnout decay and erosion of partisan linkages had less visible impact in transforming the nature of party battles in Indiana than elsewhere. More so than in most other states, through the first six decades of this century Indiana's party battles continued to exhibit aggregate characteristics markedly similar to those of the state's nineteenth-century partisan contests. Thus, a study of the historical dynamics of nineteenth-century partisanship in Indiana has much greater significance than one might initially surmise.

Melvyn Hammarberg has applied his considerable technical competence to that important task. He has made use of county-level voting and census data for all Indiana counties over the decade of the 1870s. And for nine counties of central Indiana, he has combined them with the use of a two-phased sample drawn initially from the 1870 census manuscripts and then traced to listings published in the *People's Guides*, a set of political

directories. This sampling procedure enabled Hammarberg to recover individual-level information concerning political party attachments and religious affiliations that were not collected by the census enumerators. The *Guide* sample of individual-level information comprised the core data to be analyzed statistically. The insights developed through that analysis were then tested with the use of the aggregate county-level data.

Overall, the strategy is reasonably impressive. And Hammarberg displays a keen sensitivity to the mutually dynamic interplay between data and theory, and to the distinctions between individual-level and aggregate-level analysis. All of that is commendable, and even exemplary in particular respects (especially the aggregate-level normal-vote model developed in chapter seven). Ultimately, however, the book falls well short of its potential. Its technical competence and elegantly phrased analytical formulations cannot compensate for the fact that this is a thinly researched book, and one whose major conclusions may be simply artifacts of tactical decisions made by the author.

Much of Hammarberg's analysis deals with those voters who did not identify with either of the major parties, those who held no internalized psychological sense of major-party attachment. Certainly, those listed in the *People's Guides* who called themselves "independent," or who identified with one of the minor parties, should be classified under the label "no major-party attachment." What is problematic is whether those who simply failed (or refused) to answer the question should also have been included under that rubric. Whether they should or not is no trivial matter in this instance. For of the total number in the weighted sample without major party attachment, 66.1 percent have been so classified solely because they gave no response to the party identification question (see table 3.3, p. 59). In other words, confronted with the need to handle the "no answers," Hammarberg simply operationalized an assumption that supported his hypothesis. The more acceptable research approach is the conservative assumption.

Moreover, Hammarberg fails to discuss, and perhaps to appreciate, the time-specific limitations of his core data. His sample of individual-level party preferences constitutes a snapshot frozen in time. And when the data were collected, and what configuration of attitudinal forces may have shaped the responses, are not matters that can be ignored or glossed over.

The *People's Guide* canvassers were in the field in 1873 and 1874, and the *Guides* were published in the latter year. Thus the interviews were conducted at

a point at which contemporaries had already noticed a decline in the salience of partisan attachments. President Grant, for example, in his December 1873 message to Congress observed that "political partisanship has almost ceased to exist, especially in the agricultural regions." While Grant surely exaggerated, Hammarberg's normal-vote analysis does show that the 1874 election in Indiana was marked by unusually high levels of short-term change, or deviations from underlying party attachments (see tables 7.12 and 7.13, pp. 169-71).

Were the responses given to the *Guide* canvassers reflective of the particular set of attitudinal forces prevalent in 1873-74? Or were they, as Hammarberg simply assumes, "direct measures of underlying party allegiance (p. 152)," and therefore presumably independent of the existing short-term forces? Hammarberg's conclusion that under the conditions of the 1873-79 depression the political response "of farmers was more volatile [than that of non-farmers], . . . because as political actors they were less constrained along strong party lines (pp. 179-80)" depends not only upon his data analysis but as well upon his assumption of what was being measured by the *Guide* responses. If those responses measured anything other than underlying party allegiances, then Hammarberg has observed an after the fact loosening of partisan constraints, which he has turned around and employed as a causal explanation of itself.

The body of historical materials which Hammarberg used in his research is rather thin. Apart from census and voting data, he seems to have self-consciously steered away from other categories of useful evidence. In some particulars that decision resulted in the creation of rather peculiar operational measures. A knowledge of the historical experiences of the Mission and Anti-Mission subgroups of the Indiana Baptists, for example, would have helped avoid the error of assuming that both were pietistic, and lumping them (along with other varieties of Baptists) into a single "Baptist" composite. And while the decision to avoid a traditional narrative is itself laudatory, that should not have been construed as justification for failing to use traditional types of evidence directly relevant to describing and understanding the information and cues that were available to the electorate. There are enormous pitfalls in permitting statistical rigor to serve as a substitute for studying history. For in the final analysis, Hammarberg's decision to ignore traditional documentary evidence prevented him from reconstructing party characters and group-political cultures, and thus it precluded a meaningful analysis of the historical dynamics of party allegiance.

PAUL KLEPPNER
Northern Illinois University

ROBERT B. JONES. *Tennessee at the Crossroads: The State Debt Controversy, 1870-1883*. Knoxville: University of Tennessee Press. 1977. Pp. xii, 192. \$10.95.

In the aftermath of Reconstruction no public issue caused more turmoil for Southern Democrats than the public debt—a debt carried over from the antebellum era and substantially increased by Republican regimes during Reconstruction. From Virginia where the controversy led to an anti-debt party that overthrew the Democrats to South Carolina where Democratic factions settled their differences without unduly straining their party, the fractious debt question consternated Democrats throughout the old Confederacy. The conflict in Tennessee did not lead to a separate anti-debt party, but traumatic Democratic division allowed the Republicans to regain control of the state in 1880.

Based on thorough research in Tennessee newspapers, manuscript collections, and public records, Robert B. Jones' book describes the politics surrounding the debt issue from the Democratic takeover in 1870 to the settlement of the controversial question in 1883. Jones shows clearly the tortuous problems posed for the Democrats by the debt. He sees the Democratic division over the debt as a confrontation between an urban, pro-industry, pro-debt New South group and a rural, agrarian, anti-debt group tied to the antebellum era. Thus, in Jones' view, the debt struggle in Tennessee is a microcosm of the larger struggle occurring across the South.

Although Jones has written a competent monograph, his book must be compared to Roger L. Hart's *Redeemers, Bourbons and Populists: Tennessee, 1870-1896* (1975), which devotes considerable attention to Jones' topic. Where Jones thinks his book stands in relation to Hart's, I do not know, for he makes no general comment on Hart's book in his preface or elsewhere. In analyzing and interpreting Democratic politics Jones largely follows Hart, though he does provide more detail, and he thinks economic distress more important in forming anti-debt sentiment than does Hart. In sum, I do not think that Jones' book adds significantly to Hart's analysis.

WILLIAM J. COOPER, JR.
Louisiana State University

JOSEPH H. CARTWRIGHT. *The Triumph of Jim Crow: Tennessee Race Relations in the 1880s*. Knoxville: University of Tennessee Press. 1976. Pp. xiii, 286. \$13.95.

In his study of Tennessee race relations in the 1880s Joseph H. Cartwright has skillfully explored what he calls the "brief flowering of Negro influ-

ence in Tennessee politics after Reconstruction” Inspired by William Mahone’s fusionist movement in nearby Virginia, Tennesseans who favored a modification of the crushing state debt undertook a major realignment of parties in 1881. Led by a Memphis black Republican named Edward Shaw, the movement sought to unite readjuster Democrats and black Republicans disgruntled with their parties’ firm support for the state’s debt. Blacks received few tangible rewards from the 1882 fusion campaigns, but the effort of whites to appeal to black voters opened a decade of greatly increased black political participation. By 1886, blacks—virtually powerless in the Reconstruction—were active in state and local politics and their votes were sought by whites of both parties. Although only one black Tennessean had served in the legislature during Reconstruction, a dozen held the same office in the 1880s, and limited though it was, black officeholding was significantly expanded at all levels of state and local government.

From the outset, however, black Tennesseans faced a dilemma which Cartwright perceptively describes. As a minority representing only twenty-five percent of the population, they were forced to work with whites in a political coalition. And these whites, who made up a majority in the Democratic party and a sizeable minority in the Republican party, had established clear limits on the extent of their willingness to grant concessions to blacks. Thus, when black Tennesseans increasingly demanded power and visibility within the two respective parties, the response by whites was increasingly negative.

In one of his most perceptive chapters, a study of urban blacks in the 1880s, Cartwright describes the initial successes and ultimate failures of black political leaders in Memphis, Chattanooga, and Nashville. Through skillful coalitions with both Democrats and Republicans they obtained such limited concessions as improved public services and Negro policemen. But it was in these cities in the mid-1880s that the white middle-class “reformers” led the first successful attacks against black (and poor white) political participation. Through a series of maneuvers which ranged from gerrymandering to the transfer of appointive power to the state government, urban reformers became, in Cartwright’s words, the nemesis of black political power.

In the latter part of the 1880s, a convergence of forces led to the end of this active period of black political participation. Cartwright describes and analyzes these factors: the growing political elitism of conservatives; white resentment against black “bloc voting” (though black Tennesseans were more flexible than blacks in other states); temperance advocates who blamed the failure of prohibi-

tion on black votes; and a general strengthening of racial dogmas. In 1889 and 1890, a majority of whites put aside their differences and adopted a variety of legislative stratagems to curtail black political participation.

This brief description of Cartwright’s well-researched monograph cannot do justice to the many aspects of black-white relations which he explores for the period. It is a fine first book which reminds us again of the complexity of race relations in the 1880s. The South of the immediate post-Reconstruction years may not have been “delicately poised” between white supremacy and biracial cooperation, but there was certainly more variety and flexibility than one finds in the first four decades of the twentieth century.

DAN T. CARTER
Emory University

ROGER A. FISCHER. *The Segregation Struggle in Louisiana, 1862-77*. Urbana: University of Illinois Press. 1974. Pp. xiii, 168. \$6.95.

This study of Reconstruction in Louisiana focuses on the central issues of segregation in the public schools and segregation in public accommodations. The author’s thesis is that Louisiana was unique among Southern states during Reconstruction. Whereas in other Southern states Reconstruction was dominated by whites who were often more interested in the spoils of office, in Louisiana there was a “black power movement in miniature.” This occurred because of the large prewar population of free *gens de couleur*, who gave Louisiana blacks a group of capable people able to assume positions of leadership for the race. These aristocrats quickly realized that their own future depended on a unified “black” position representing blacks of all classes. As delegates to the state constitutional convention they insisted on full civil equality for blacks, and the constitution of 1868 is testimony to their ability. It provided for the black vote, the right of blacks to hold office, non-segregated education and non-segregated public accommodations. Had it not been for these black leaders, the former slaves would have “settled for considerably less.”

Robert Fischer’s claim that black power in Louisiana was unique is open to question. Too many recent studies have revealed the extensive black participation in other Reconstruction states. Also, Fischer offers evidence to contradict his contention that but for the *gens de couleur* the black masses would have been content with something less than an integrated society. Throughout his study, the author shows that while the black elite worked to achieve legal equality, the black masses actively worked for the identical goals. It was the angry

black mobs who forced compliance with non-segregation when they refused to ride on black-designated streetcars and climbed aboard white cars. And when black students were assigned to segregated schools, it was the black masses who armed themselves and made integrated schools in New Orleans a reality. Without this action in the streets by the masses, the constitutional achievements of the black elite would have been meaningless.

Fischer's account of the segregation struggle in Louisiana is informative and well done. Perhaps more about the role of the black masses as well as the black elite would give us a better understanding of the accomplishments of black power. If the masses were more interested in bread and butter issues, they were still no less willing to fight for their civil equality than were the *gens de couleur*.

LEE FINKLE

Indiana University-Purdue University,
Indianapolis

CHARLES WOLLENBERG. *All Deliberate Speed: Segregation and Exclusion in California Schools, 1855-1975*. Berkeley and Los Angeles: University of California Press. 1977. Pp. 201. \$10.00.

After reading this book, one has the distinct impression that California has excluded and segregated more racial groups from its schools than any other state. Charles Wollenberg's story begins in the 1850s, when black parents in San Francisco appealed to the city for public schooling for their children. The city responded by establishing California's first segregated "colored school." This action was followed by the growth of a colored school system in California which remained in existence until 1890 when the State Supreme Court ruled against the existence of segregated schools for blacks. Segregation of black people in the schools, of course, continued through the actions of local school districts.

Two of the other major racial groups to be excluded from the schools were Chinese and Japanese. California's first Chinese school was opened in San Francisco in 1859 following requests by members of the Chinese community for public schooling. In 1884 the San Francisco School Board forbade the admission of Chinese to the public schools. At the time, the California constitution declared Chinese dangerous to the well-being of the state. The resolution of this issue in the courts resulted in the establishment of a segregated Chinese school.

The author's treatment of the segregation of Japanese students shows the linkage between school issues and foreign policy. The issue of segregation of Japanese resulted in the Japanese gov-

ernment putting pressure on President Theodore Roosevelt, who in turn put pressure on California school officials to provide education for Japanese children. The incident was similar to the international embarrassment caused by segregated schools in the South during the 1950s.

The book also traces the exclusion and segregation from the schools of Indians and Mexican-Americans. As with the other groups discussed in this volume, the problem of the segregation of the children of these minorities continues to the present. The author traces the history of these issues to 1975.

The one major weakness of the volume is the lack of any attempt to explain why racial prejudice and the resulting issue of segregated schools developed in California. One explanation, which seems inherent in the issues raised but not explored by the writer, is the link between exploitation and segregation. In all cases racial prejudice appears to be used as a rationalization for some form of economic exploitation. Racial prejudice and theories of racial inferiority provided a justification for the slavery of blacks and the taking of land from the Indians. Theories of inferiority and prejudice also justified the economic exploitation of the labor of Chinese, Japanese, and Mexican-Americans. A common weakness of histories of education is the lack of connection between the history of the school and the structure of society. This volume on school segregation in California would have been strengthened if it had been placed in the broader framework of the social and economic development of California.

JOEL SPRING

University of Cincinnati

AUGUST MEIER and ELLIOTT RUDWICK. *Along the Color Line: Explorations in the Black Experience*. (Blacks in the New World.) Urbana: University of Illinois Press. 1977. Pp. xvi, 404. \$14.00.

August Meier and Elliott Rudwick are among the most accomplished historians of the modern black experience in America. Thus, the appearance of this new volume of their essays, the tenth volume in the "Blacks in the New World" series edited by Meier, is a significant event. Eleven of the articles have appeared previously over the last twenty-five years. The remaining three essays are published in this volume for the first time. The central theme of the work is organized black protest and the many forms it has taken from the time of Frederick Douglass through the 1960s. The essays are arranged topically into three sections dealing with Afro-American leadership, black nationalism and black power, and the history of nonviolent protest.

Part one of the collection offers perceptive analyses of the careers of Douglass, W. E. B. Du Bois, Booker T. Washington, and Martin Luther King. Some of these essays are pioneer studies that have been updated by Meier, Rudwick, Louis Harlan, and others in later works; yet they are still quite valuable to have in one volume. The highlights of this first portion of the work are two ground-breaking essays on the shift from white to black leadership in the NAACP. The first, "The Rise of the Black Secretariat in the NAACP, 1909-1935," covers some of the same bureaucratic ground that Charles Flint Kellogg did in his work on the organization up to 1920. Meier and Rudwick continue the story through the 1930s when James Weldon Johnson and Walter White rooted the NAACP in the black community and made it less dependent upon white liberal philanthropy. The second essay, "Attorneys Black and White," which first appeared in the *Journal of American History*, follows the similar change in the NAACP's legal development. From an original dependence upon the likes of Clarence Darrow and Moorfield Storey for legal representation, the black organization turned in the 1930s to William Hastie and Charles Houston. Both articles show clearly that when society began to respond to demands for black rights, the whites who were at first desperately needed to give the NAACP money, prestige, and legitimacy were given a lesser role in organization activities. By the 1930s, with an entrenched black bureaucracy controlled by the all-powerful Walter White and a legal department headed by Houston and peopled by his Howard students, the NAACP, more confidently and purposefully than it had previously done in the shadow of whites, continued the struggle for equality.

Part two also contains a new and important essay. "Integration vs. Separatism" discusses NAACP and CORE responses to the increased militancy of the sixties. The essay is particularly significant for its contrasting of the two race organizations and incisive portraits of their leaders. The continuing clash between the older, more centralized and legalistic NAACP under organization-man Roy Wilkins, and the more activist, loosely structured CORE under the charismatic James Farmer is better developed here than in the Meier and Rudwick 1973 work on CORE. The insights offered in this essay were, for the most part, lost in a mass of organizational detail in the CORE volume.

The last section of the book features a nearly one-hundred-page "note" on the origins of non-violent direct action in Afro-American protest that sometimes taxes the reader's attention with more detail than is necessary. The authors trace the history of nonviolent action back into the nine-

teenth century when Frederick Douglass led protests against Jim Crow streetcars in Massachusetts and southern blacks boycotted segregated trolleys. In bringing the history into the twentieth century, Meier and Rudwick clearly puncture the myth that black nonviolent protest was a concerted effort led by individuals aware of the historical traditions of black protest and schooled in Gandhian tactics. Nonviolent direct action protest from the antebellum period to the 1960s was, they point out, "episodic and marked by sharp discontinuities." Few leaders knew much about the protests of earlier periods, and with all due respect to Gandhi, important instances of nonviolent direct action came spontaneously when black people "faced a serious loss of status or when they experienced a rising set of expectations."

Along the Color Line, like Du Bois' *Crisis* column of the same name, makes some important contributions to Afro-American history. It does fail to relate the organizational history of the NAACP and CORE to what was happening elsewhere in the Progressive Era and the turbulent 1960s and the final "note" could have made its case with less minutiae. Still, these few quibbles do not measurably detract from a work which reflects the growth and development of black protest in American and that of two successful interpreters of black history.

ANDREW BUNI
Boston College

LAWRENCE W. LEVINE. *Black Culture and Black Consciousness: Afro-American Folk Thought from Slavery to Freedom*. New York: Oxford University Press. 1977. Pp. xx, 522. \$15.95.

When Amiri Baraka (then LeRoi Jones) maintained, in his groundbreaking 1963 work, *Blues People: A History of Negro Music in White America*, that "the most expressive Negro music of any given period will be an exact reflection of what the Negro himself is[,] . . . a portrait of the Negro in America at that particular time," the gauntlet would seem to have been thrown down. For all of that, however, historians of the Afro-American experience were generally content to ignore the challenge implicit in Baraka's dictum and continue working with more or less conventional materials in more or less conventional ways. Thus, with a few honorable exceptions, it has to date remained for the anthropologists, sociologists, ethnomusicologists, and the like to put Baraka's assertion to the test and attempt its extension into other areas of folk culture.

It is doubtful in the extreme that such indifference on the part of historians to the black folk legacy will persist unaltered after the contents of

Lawrence Levine's long-needed study of "the materials of Afro-American oral expressive culture" have been thoroughly digested. Drawing with an impressive command upon a wide variety of those "folk sources without which it is impossible to understand the history of black Americans"—sources that range from the devout texts of the spirituals to the rather earthier ones of the "dozens," the "toasts," and the feats of such legendary figures as Stagolee and Shine—Levine seeks to answer the questions: "What were the contours of slave folk thought on the eve of emancipation, and what were the effects of freedom upon that thought?" For the most part he is markedly successful, in the process providing ample empirical verification of Baraka's earlier hypothesis.

Levine believes that by 1860, if not before, the enslaved Afro-American majority had "created a new world by transcending the narrow confines of the one in which they were forced to live" (p. 32). The clearest ideological expression of this transcendence is contained in the communally-fashioned spirituals, in which the "most persistent single image . . . is that of [the slaves as] the chosen people" (p. 33). Following the destruction of slavery there emerged a new cultural synthesis that combined elements born during bondage with those wrought in freedom, elements of African descent with those absorbed from non-black society. One of the most vivid expressions of this synthesis—but by no means the sole one—is the body of trickster tales, in which a smaller, weaker figure outwits and ultimately defeats a larger and seemingly more formidable opponent. The ubiquity of such tales well into this century has patent relevance to the actual situation in which Afro-Americans found themselves long after the demise of formal slavery.

In more recent times, though, many black people have begun to eschew the trickster-protagonist in favor of (to employ the title of Levine's sixth chapter) "A Pantheon of Heroes." Whether real (Jack Johnson, Joe Lewis) or mythic (John Henry, Shine), the members of this pantheon prefer to rely on direct confrontation rather than guile in their ordeals and travails. As Levine remarks, this and other changes "taking place in black folklore" have their origins in those developments "occurring in black life as well"; they have "reflected rather than created the mood of increasing numbers of Negro Americans" (p. 438).

Running as counterpoint to the foregoing themes, which are here far too baldly summarized, is Levine's decisive refutation—based on data rather than dogma—of the parallel and related notions that black people in the U.S. were wholly infantilized, depersonalized, and "Sambo-ized" by the ravages of chattel slavery; and that, accord-

ingly, their contemporary folk culture must be regarded as "pathological" in nature. On the contrary, the weight of the evidence derived from oral folk culture "indicates that, [although] the slave system may have diminished the central communality that had bound African societies together, it was never able to destroy it totally or to leave the individual atomized and psychically defenseless" before white onslaughts.

Levine's monograph, as monumental an addition to the field as it is an innovative one, equals in significance any work on Afro-American history of the preceding decade. It is most unlikely that study of the Afro-American past will ever be able to resume its former aloofness toward the rich resources for the historian of black folk culture. Nonetheless, his volume is not without its flaws. Although generally graceful and lucid, the writing is at a few points awkward or even ungrammatical. Of a more serious nature, Levine does not give the impression of having investigated in great depth certain important aspects of oral folk culture in this century, including, *inter alia*, recorded sermons, children's rhymes, games and rhythmic play patterns (apart from the "dozens"). Finally, while the number of other historical researchers who have explored Afro-American folk materials is admittedly small, the development of his theoretical frame of reference probably owes more to them (principally, but not exclusively, Baraka) than most nonspecialist readers will be able to surmise from Levine's words alone.

FRANK KOFSKY
California State University,
Sacramento

IRWIN YELLOWITZ. *Industrialization and the American Labor Movement, 1850-1900*. (Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1977. Pp. 183. \$12.50.

"How did organized workers, with the skills and expectations of a preindustrial society, react to economic changes that virtually reconstructed the nation and the role of labor within it?" Having posed this problem, Irwin Yellowitz answers it, first by reducing those changes to the mechanization of production, and then by pragmatically sorting out the problems which mechanization posed for late nineteenth-century trade unionists into discrete categories (unemployment, status, overproduction, child labor, etc.) Using this method he has produced a useful summary of the efforts of various unions to cope with technological change. The unions responded in a number of ways: coalminers' protracted struggles to inhibit overproduction, not simply by individual miners but

by all the colliers of large marketing regions; the campaign by blowers of lamp chimneys to prevent the introduction of crimping machines from accelerating the restrained, steady work pace which their union rules had prescribed; and the cigar makers' half-century attempt to control the impact of wooden molds on the division of labor within their shops. Much of the information reminds the reader of George E. Barnett, *Chapters on Machinery and Labor* (1926), though Yellowitz's analysis does not achieve the level of sophistication which characterized Barnett's classic.

Although a chapter on immigration offers the provocative thesis that trade unionists had become increasingly anxious about the prospect that their accomplishments would be swamped by the export of Europe's poverty decades before that anxiety became chauvinistically focused on "New Immigrants," those dealing with female and child labor are thin and unimaginative. The discussion of apprenticeship is welcome, because the topic has been neglected since Paul H. Douglas published *American Apprenticeship and Industrial Education* in 1921, but the contrast between the failure of most trades and the simultaneous success of many professions in securing formal, and even legal, control over entry into their ranks calls for a more systematic scrutiny than is offered here.

Yellowitz's conception of the trade unions' response to mechanization is informed by the ideology of Progressivism. "Failure to develop a strong welfare state left no mechanism by which divergent interests might be reconciled with the needs of a larger public," he argues, and consequently the protection of workers against the pitfalls of technological progress was left to trade unions, which were "ill-suited for such a task" (p. 94). Nevertheless, even those of us who remain unconvinced that the twentieth-century state has converted rising productivity into a source of unqualified benefits to workers will find in this book evidence of the fundamental level on which nineteenth-century trade unionists repudiated the governing political economy of our age, as well as their own. They insisted, as Yellowitz says, "on a fair economy rather than an efficient one."

DAVID MONTGOMERY
University of Pittsburgh

MANSEL G. BLACKFORD. *The Politics of Business in California, 1890-1920*. Columbus: Ohio State University Press. 1977. Pp. xi, 221. \$12.50.

Taking a cue from Robert Wiebe's *The Search for Order*, Mansel Blackford examines the search for order by California businessmen between 1890 and 1920 and their influence on state policies. Black-

ford focuses on various sectors including agriculture, petroleum producers, lumber interests, investment banks, and insurance firms. In each instance Blackford discusses the economic needs that led particular segments in these industries to seek further integration and coordination in the hope of achieving greater economic stability. Since such private efforts often proved insufficient, the various interests worked through legislative lobbies or trade associations to influence state policies to attain their goals. Their endeavors, Blackford notes, were only partially successful. The author feels that other historians have not treated the subject of business-government relations adequately. Alfred Chandler, Jr. "missed much of the significance of the social and political environments within which businessmen operate," Gabriel Kolko's "interpretation is simplistic," and the conclusions of George Mowry and Spencer Olin "need to be reexamined" (pp. x-xi). And Blackford also expects that his book will help political scientists and sociologists understand "the extent to which modern American government has been pluralistic or elitist in nature."

Unfortunately, despite these claims and occasional loose use of social science jargon, the author does not deliver on his promise to break new ground. Lack of depth and dearth of insight are the most striking characteristics of the volume. It is puzzling why Blackford did not delve more deeply into unpublished primary source materials, especially manuscript collections and oral history depositories. These would have increased immeasurably the significance of his book. The present study is based largely on printed materials such as newspapers, periodicals, and state documents, many of which have already been exploited by others. Consequently the book is a depository of familiar facts rather than an innovative analysis. At times the author's lack of perception is striking. In discussing the movement for railroad regulation in California between 1890 and 1920, for example, Blackford recapitulates well-known influences, but curiously omits any mention of the Panama Canal. And yet the completion of the Panama Canal had major consequences for California's economy, its transportation system, and public policy. Its impact can hardly be ignored completely.

The author has clearly made a strenuous effort. One hopes he will continue to investigate the subject, which he quite rightly notes deserves further analysis.

GERALD D. NASH
University of New Mexico

PETER J. FREDERICK. *Knights of the Golden Rule: The Intellectual as Christian Social Reformer in the 1890s*.

Lexington: University Press of Kentucky. 1976. Pp. xvi, 323. \$18.75.

From the welter of Gilded Age reform, Peter J. Frederick has chosen ten "knights" distinguished by their passionate devotion to the Golden Rule as a basis for reforming capitalist industrial society. Frederick believes these Christian social reformers espoused ethical commitments and a social program similar to those of intellectuals in his own generation who participated in the civil rights movement and responded to the tragedy of Viet Nam. In this study, the decision for or against political activism becomes the central dilemma faced by knights of the Golden Rule.

Influenced by American democratic philosophers, these men were more profoundly touched by intellectuals in the European radical tradition, especially Mazzini, Ruskin, and Tolstoy. Socialists or anarchists with one exception, they rejected Marxist principles regarding class conflict and revolution. Rather, they believed a new society could evolve peacefully, as people were persuaded to substitute brotherly, social feelings for selfish individualism. Through writing, teaching, preaching, and organizing, mostly within the middle class, the knights labored to bring forth the kingdom. By Frederick's standard they were nearly all failures, precisely because they were unable to cut middle-class ties and become activists in the cause of the working class. Henry Demarest Lloyd partially vindicated himself by leading a miners' strike and defending the Haymarket anarchists and Eugene Debs. Sam "Golden Rule" Jones, the book's hero, brought Christian pacifist ideals to concrete application as reform mayor of Toledo.

A fascinating element of this study is its juxtaposition of people not ordinarily classified together. We expect to find William Dean Howells with genteel reformers, Lloyd with muckrakers, George Herron and Walter Rauschenbusch with Social Gospelers, Vida Scudder among settlement-house workers, William D. P. Bliss and Benjamin O. Flower with reform editors, Edwin Markham and Ernest Crosby among shapers of literary realism, and "Golden Rule" Jones as a prototype of urban progressives. By organizing people according to belief rather than function, Frederick reasserts the motive force of ideas and reminds us that identical influences can inspire people in widely different walks of life to strive for the same ideal.

Yet placing so little importance on function creates a logical problem. Frederick explains most of his knights' failure to become activists by referring to personal shortcomings: indecisiveness, snobbishness, excessive ambition, or insufficient courage to give up the comfort and security of middle-class life. Another explanation lies in their

careers. Most of the knights were indeed activists, of a different sort—intensely engaged in creating roles and building institutions that would characterize the modern intellectual life of an urban, industrial society. Acutely conscious of their part in shaping both roles and opinion, they made choices with an eye to the probable result of contemplated actions. From boyhood Howells, for example, tracked himself into literature, where he almost certainly achieved greater influence as the author-editor who could do most to influence emerging standards of literary taste than he might have leading a strike. The guilt that Frederick's knights expressed was a complex mixture of decency, frustration, nostalgia for a simpler time when roles were less restrictive, and new-middle-class awareness that effective intellectual independence required not only the greatest prudence in action but also financial independence of the old-middle-class sort that Lloyd, Crosby, and Jones alone possessed.

Frederick argues, correctly, that unlike Christopher Lasch's new radicals the Christian social reformers were not alienated from the middle class. It is odd that he finds their failure to renounce a middle-class ethos surprising. Howells and the others hoped that a reformed capitalism or voluntary socialism would extend to all Americans the opportunities and values the Gilded Age reserved for the middle class. These reformers believed that reform leadership had to come from the middle class. Their compassion for working-class suffering and profound indignation at the system that caused it should not be confused with the contempt for bourgeois culture that has characterized a segment of the intellectual community since the 1920s.

Such disagreements negate neither the profit nor the pleasure of reading a volume so imaginatively conceived, elegantly written, and competently edited as this one.

MARY O. FURNER
Northern Illinois University

NINA DRAXTON. *Kristofer Janson in America*. Boston: Twayne Publishers, for the Norwegian-American Historical Association. 1976. Pp. xiii, 401. \$10.95.

Few immigrants to the United States arrived with so brilliant a reputation preceding them as did Kristofer Janson in 1881. One of Norway's major poets—along with Bjørnsterne Bjørnson, Ibsen, and Jonas Lie—he was ordained a Unitarian minister in Chicago and moved on to Minnesota. Nina Draxton's excellent biography makes a major contribution to ethnic history in illuminating the magnetic and many-sided Janson. Liberal, literary,

intellectual, mesmeric, and non-Lutheran, Janson Unitarianized a remarkable number of immigrants in the heart of Norwegian-Lutheran orthodoxy. Attracting non-immigrants as well, he built Nazareth Church in Minneapolis and Nora Free Church in Hanska and established congregations in St. Paul and Underwood, Minnesota and Hudson, Wisconsin. Nazareth Church beamed brightly for a decade as a beacon of enlightenment for the community at large. It served as a vital link between the old and new worlds and as a forum for social protest, readings of Janson and other Norwegian authors, debates, concerts, and theatricals.

The Janson home and church fostered the exchange of ideas on politics, women's rights, temperance, religion, industrialization, and urbanization. *Saamanden* (*The Sower*), a magazine founded by Janson, purveyed his philosophy which was strongly influenced by Henry George, Edward Bellamy, and the Christian socialists. An increasing absorption with spiritualism brought the demise of his career in America. He and his wife, Drude Krog drifted apart because of his infatuation with Louise Benson, a spiritualist whom he invited to live in their home. Drude, overworked and disliking city life, twice fell in love with younger men, one whom was Knut Hamson, her husband's secretary for a time. An author in her own right, she was overshadowed by the cult that enveloped her charismatic husband.

Nina Draxton has dusted off a figure and era of seminal importance to the political and intellectual life of the Upper Midwest. The Jansons infused liberal thought into the mix of native and immigrant cultures at a time when Minneapolis was emerging as the metropolis of the Northwest. Though their personal American adventure ended after twelve years, they made a lasting contribution to the progressive tradition that took root prior to their emigration to Minnesota and blossomed forth after their departure in 1893. Draxton's mastery of sources and well-written narrative show their odyssey to have been a sad one for Kristofer and Drude. Disenchanted by their personal problems and experiences in the New Scandinavia of Minnesota, so optimistically boosted by other immigrants, they returned to Europe. Janson devoted the rest of his life to the workers' educational movement in Norway. His congregations disappeared almost as soon as he, except for Nora Free Church in rural Minnesota which flourishes today. This book is the most recent publication of the Norwegian-American Historical Association, whose record for interpreting the story of a single ethnic group is without equal in the United States.

RUSSELL W. FRIDLEY
Minnesota Historical Society

J. WADE CARUTHERS. *Octavius Brooks Frothingham, Gentle Radical*. University: University of Alabama Press. 1977. Pp. ix, 279. \$15.00.

This is the first book-length biography of Octavius Brooks Frothingham (1822-95), a major figure in the development of radical religion in late nineteenth-century America. J. Wade Caruthers correctly demonstrates that his subject was a significant force in reshaping Unitarianism in accord with the demands of modern scientific thought. In rather traditional fashion, it traces Frothingham from his childhood as the son of a prominent, conservative Unitarian pastor through his Harvard years, his involvement in the Transcendentalist movement, his pastoral work in Salem, Jersey City, and New York City, and his crucial participation in the Free Religious Association, to his ultimate retirement and death. It also treats Frothingham's career as a theologian, journalist, biographer, and historian. By the end, Frothingham emerges as an extremely talented thinker whose life emphasizes again the intellectual vitality which existed during the Gilded Age.

Unfortunately, this biography does not do justice to its subject. Frothingham's personal papers were destroyed, so any analysis of his private life must remain limited. Caruthers recognizes this and claims to have written what is essentially "an intellectual biography." But it is as intellectual history that the book is weakest. Frothingham's ideas are summarized, but not clearly related to those of other thinkers. The influence of Theodore Parker is never precisely defined (significantly, not one of Parker's theological works is cited). Herbert Spencer, the other major influence on Frothingham, is scarcely mentioned. More damaging, Frothingham's own major work *The Religion of Humanity* (which Stow Persons called "the representative essay in free religion") gets only a passing reference in the text and another in one footnote. Consequently, descriptions of key ideas such as "Concepts of God," "Death and Immortality," "Jesus and Christ," or "Sin and Social Ethics" are incomplete and misleading. Caruthers does not really seem comfortable discussing theology and philosophy. He is better at dealing with Frothingham's historical writings, his statements on public issues, or his various organizational activities. Perhaps it would have been more accurate to describe this as a public, rather than an intellectual, biography.

Lastly, it is necessary to report that this book suffers from an incredibly shoddy job of editing. There are excessively long quotations, occasionally extending onto a third page. At places the writing is redundant or contradictory. Most exasperating is the regular garbling of proper names almost

beyond recognition and certainly beyond anything that can be blamed on the typesetter. John White Chadwick, who figures prominently in the book, is referred to as William White Chadwick everywhere except under his photograph. James Freeman Clarke appears as John Freeman Clark, David Friedrich Strauss as Frederiech C. Straus, John Fiske as Fisk, George Ripley as Riply, John Stuart Mill as Mills, and James McCosh as Reverend McGoch. This is merely a small sample; similar errors extend by the score throughout the text and then spill over into the index, where they create alphabetical chaos in an already thin tool. In the notes, Harvard's Widener Library is consistently cited as "Widner." There are also blanks left in several dates, presumably with the unfulfilled intention of inserting a number later. It is a quite embarrassing display.

CHARLES D. CASHDOLLAR
Indiana University of Pennsylvania

THOMAS KESSNER. *The Golden Door: Italian and Jewish Immigrant Mobility in New York City, 1880-1915*. (Urban Life in America Series.) New York: Oxford University Press. 1977. Pp. xxvi, 224. Cloth \$12.95, paper \$3.50.

With Thomas Kessner's analysis of turn-of-the-century occupational and residential change among two "new immigrant" groups, systematic historical study of American social mobility has finally bitten into a portion of the Big Apple; the results have a familiar taste. Blue to white collar advancement rates even higher than those discovered elsewhere lead the author to conclude that New York, too, "afforded immigrants and their children a comfortable margin of mobility" (p. 176). Though both advanced notably, Kessner verifies the common assumption that "the golden door" opened wider for first- and second-generation Jews than for Italians. Jews typically entered the labor market at higher levels and exhibited more rapid upward mobility, less skidding, and distinctive career paths. The study also confirms the notion of immigrant concentrations as porous "launching pads" for mobility rather than closed ghettos.

These conclusions rest upon household samples drawn from 1880, 1892, and 1905 manuscript censuses linked to directory data permitting cross-sectional and longitudinal analysis. Statistical tabulations are presented within a narrative rather than methodological framework and are wrapped in a wealth of illuminating detail to produce a far more readable, interesting book than others in the genre. But stylistic grace and perceptive insight prove insufficient to counterbalance a parsimonious and superficial statistical analysis which fails to take advantage of the explanatory potential of the quantitative approach and limits the applicability of the findings.

Sample data are used only to chart certain simple trends in gross occupational profiles, first- and second-generation occupational mobility, and residential change. Though Kessner is concerned to explain resulting group differentials, there is virtually no attempt to relate change in one variable with change in another or to exploit the full range of available census data. For example, possible links between household employment patterns and mobility, parental mobility and mobility of children, or occupational and residential change remain unprobed as Kessner turns for explanation instead to what he terms "common-sense observations" (p. 94) derived from contemporaneous and secondary sources. The conclusions—as in arguments for cultural causation of mobility differentials or the effects of neighborhood on mobility—may indeed make sense but cannot be regarded as proven by the data. Nor are the exceptionally low rates of persistence satisfactorily integrated into the analysis.

Moreover, brusque presentation of sample procedures, obscurely constructed tables, and the lack of maps or systematic description of residential patterns in the confusing but potentially valuable penultimate chapter are other problems complicating interpretation. Kessner is clearly at his best in his effective narrative recasting of the familiar saga of the New York immigrant, but the half-hearted quantification makes much of that recasting resemble padding on an article-length core of significant analysis.

KATHLEEN NEILS CONZEN
University of Chicago

MARY ROTH WALSH. *"Doctors Wanted: No Women Need Apply": Sexual Barriers in the Medical Profession, 1835-1975*. New Haven: Yale University Press. 1977. Pp. xxiii, 303. \$15.00.

Among the growing number of significant books which have been published in women's history in recent years is Mary Roth Walsh's *"Doctors Wanted: No Women Need Apply."* This work helps to fill a large gap in American women's history by demonstrating how "the medical establishment made a conscious effort to minimize the number of women physicians" over the past century and a half (p. xviii).

Walsh challenges the thesis put forth by Gerda Lerner, Ann Douglas Wood, Janet Donegan, and others that "female physicians were sacrificed on the altar of professionalization" during the nineteenth century (p. 10). She argues rather that, with

the exception of midwifery, women did not play a significant role in colonial medicine. Furthermore, she suggests that possibly "professionalization of-fer[ed] women their first real opportunity in medicine" by providing them with a series of known obstacles to overcome. In fact, Walsh states that the "golden age" of women physicians in the United States occurred during the latter years of the nineteenth century, at the precise time when the forces of professionalization should have been working to keep women out of medicine.

Despite her stated thesis, there are times when Walsh seems unsure as to whether or not the professionalization of medicine actually helped or hindered women's entry into and advancement in medicine. For example, early in the book she minimizes the role that medical licensing played in barring women's entry into medicine by stating that "virtually all medical licensing during the first half of the nineteenth century was honorific. . . ." (p. 12). Later, however, she describes how Boston's female physicians placed special importance on membership in the Massachusetts Medical Society because during the "first half of the nineteenth century, that body controlled medical licensing in the state . . . [and] the society's certification procedures were viewed as sifting out the regularly educated physicians from the sectarians and poorly educated practitioners" (p. 150).

Walsh is at her best when discussing the barriers to women physicians' advancement in Boston during the years, 1850-1900. Her chapters on Samuel Gregory's paternalistic attitude toward women and his clash with Marie Zakrzewska are outstanding. Moreover, Walsh resurrects Zakrzewska from near oblivion by focusing on her successful efforts to establish and manage the New England Hospital for Women and Children.

Unfortunately, Walsh does not provide this same type of in-depth examination for other areas of the United States. For example, she devotes only a few paragraphs to a discussion of the opening of Johns Hopkins University's medical school to women. Similarly, she gives only brief attention to the Woman's Medical College of Pennsylvania and the challenge which it presented to the predominately male medical establishment. Walsh justifies the Boston focus of her study by arguing that "the struggle of women to enter medicine in this particular city presaged that of their sisters on the national scene" (p. xiv). She does not, however, satisfactorily substantiate this point.

For the most part, Walsh's book is solidly researched and well written. It should prove of interest and value to both professional historians and contemporary feminists.

JUDY BARRETT LITOFF
Bryant College

G. MICHAEL MCCARTHY. *Hour of Trial: The Conservation Conflict in Colorado and the West, 1891-1907*. Norman: University of Oklahoma Press. 1977. Pp. xvi, 327. \$12.50.

G. Michael McCarthy argues persuasively in this well-written, solid, political history that Colorado's "hour of trial" in the late nineteenth and early twentieth centuries was typical of the history of conservation in the American West. Certain characteristics of the conservation controversy appeared and reappeared: animosity toward the East and inter-regional quarreling over the disposition of land and resources; anticonservationist sentiment bisecting class lines; a strident, noisy, anti-conservationist press and political leadership that veiled the fact that conservationists (and the apathetic) outnumbered their opponents; and, most important, the main motivation on either side was economic, not esthetic.

The story is told as a political debate over forest withdrawals and grazing policy. Minerals receive less attention, and wildlife, none. The author relies heavily on contemporary newspapers, and this adds a dramatic if not always scientific flavor to the narrative. In the last chapter, McCarthy takes a bold leap in time from the first decade of the twentieth century to Colorado's present environmental controversies, but connections between early events, issues and their interpretation, and today's debate are not clear. The problem of relevance is not resolved. Withal, the skillful integration of earlier controversies in Colorado with what was simultaneously happening elsewhere in the nation make this volume a valuable contribution to American environmental history.

MORGAN SHERWOOD
University of California,
Davis

RICHARD HUME WERKING. *The Master Architects: Building the United States Foreign Service, 1890-1913*. Lexington: University Press of Kentucky. 1977. Pp. xvi, 330. \$18.75.

It has become a commonplace of recent studies of American foreign policy that bureaucratic politics often determine diplomatic actions. Historians and political scientists argue over just when bureaucratic competition began to set the limits of foreign policy, and they have difficulty selecting actual cases in which they can show functionaries battling to control political outcomes. Graham Allison's study of the Cuban missile crisis, *The Essence of Decision*, sets the standard to which other books aspire but rarely reach.

Richard Hume Werking's study of the develop

ment of the modern American foreign service explicitly attempts to set the origins of bureaucratic rivalries in foreign policy as early as possible. Werking has not challenged the pre-eminence of Allison's book, but he has written a straightforward account of the movement for a professional foreign service in the early twentieth century. Briefly, *The Master Architects* argues that the aims of diplomats, consuls, Commerce Department officials, businessmen, and politicians shaped the foreign service in the years before the First World War. He suggests that subsequent reforms of the 1920s, 40s, 50s, and 70s had their roots in battles of the Roosevelt and Taft administrations.

Werking is at his best in his descriptions of the men who first argued for foreign service reform. He has made good use of the relevant manuscript sources, and his portraits of the reformers are lively and colorful. He has even spiced his book with a few rarely seen photographs of these fellows. His account of the Commerce Department's rivalry with the State Department over control of foreign reporting on commercial matters tells a story which other writers in this field, myself included, have neglected.

And yet there are serious flaws in Werking's approach. The title indicates what is wrong, for surely the phrase "master architects" should be used ironically. The structure they built had a weak foundation, and it certainly has been inconvenient to get around inside of it. Remodelers have been at work on it ever since; many observers who have looked at the sprawl of agencies inside the State Department have despairingly concluded that the only thing to do with the place is to tear it down and start all over. Part of the reason for the continuing demand for revisions in the organization of the foreign service comes from the fact that its structure never was the result of a single, coherent plan drawn up by men who agreed on a vision of what the service should look like. Instead, the service which emerged in the last seventy years is the result of numerous contending designs for a foreign affairs bureaucracy. At times, Werking seems aware of the fact, if not the intensity, of the struggle among the various interests that wanted a new State Department. But *The Master Architects* overlooks the sense of passion which has so obviously infused the debates on foreign service reform for seventy years. Werking ascribes to the participants in the debate a foresight and rationalism which they simply did not have; hopes, dreams, jealousies, and lust for professional advancement the master architects possessed in abundance, but real blueprints have always been missing.

ROBERT D. SCHULZINGER
University of Colorado,
Boulder

DELBER L. MCKEE. *Chinese Exclusion Versus the Open Door Policy, 1900-1906: Clashes over China Policy in the Roosevelt Era*. Detroit: Wayne State University Press. 1977. Pp. 292. \$17.95.

Americans tend to identify the Open Door as the traditional United States policy toward China. Delber McKee reminds us that for many years America had a second policy—the exclusion of the Chinese from the United States. Although a few individuals had earlier perceived the inherent contradictions, the coexistence of the two policies at the turn of the century was shattered by two major developments during the years 1900-06. First, the anti-Chinese forces implemented the Powderly policy, which based exclusion on race instead of class (laborers), with the aim of forcing the Chinese to leave America. Second, China unexpectedly resisted by renouncing the immigration treaty of 1894 and instituting a boycott against American goods. McKee shows that when the Open Door forces and exclusionists clashed over the exclusion laws of 1902 and 1904 and the abortive Foster bill of 1906, the exclusionists won resounding victories. President Theodore Roosevelt, pragmatic and political, also bowed to the exclusionists in 1902 and 1904. But when he was pressed by the Open Door forces and confronted with the Chinese boycott, Roosevelt used administrative action to shift the exclusion policy back to the class-oriented approach and to rectify the abuses of the Bureau of Immigration. Nevertheless, the Open Door suffered immediate and permanent damage in the form of reduced trade and cultural ties. And even if Americans had overcome the problems of capital investment and the peculiarities of the China market, McKee argues that "the exclusion policy would have remained a handicap of some consequence" (p. 220).

McKee correctly stresses this darker side of Sino-American relations. By demonstrating the indifference of many Americans to the China trade, he underscores the problems facing the advocates of the China market and the purveyors of Open Door rhetoric. By resting his case on the votes on several congressional bills, however, he overstates the importance of exclusion on China policy, even during the Theodore Roosevelt administration. It was not the equal of the Open Door. As McKee notes, Roosevelt and the Open Door interests recognized that Peking's weakness enabled the United States to have both modified exclusion and the China market. Nor did exclusion deter the Chinese from seeking United States cooperation to protect its Manchurian frontier in 1907-08 or to undermine the spheres of influence during the Wilson-Reinsch era. A decade later, Americans blamed their difficulties in realizing the Open

Door on the contemporary fiasco of the American China Development Company; they did not mention the exclusion policy.

Still, McKee has made an important contribution in this well-written and ably researched monograph, which will interest ethnic as well as diplomatic historians. He has presented a comprehensive account of the evolution of Chinese exclusion policy during the Roosevelt administration and a thorough analysis of the nature, methods, and influence of the exclusion forces. Although additional work in Chinese sources may be needed, he makes a case for the role of Chinese-Americans in initiating the boycott. It is doubtful if Chinese exclusion had as great an effect on American diplomacy as did Japanese exclusion. But after the appearance of McKee's study, it will no longer be possible for diplomatic historians to ignore Chinese exclusion as a factor in Sino-American relations.

NOEL PUGACH
University of New Mexico

MICHAEL J. HOGAN. *Informal Entente: The Private Structure of Cooperation in Anglo-American Economic Diplomacy, 1918-1928*. Columbia: University of Missouri Press. 1977. Pp. viii, 254. \$12.50.

Recently historians have subjected America's overseas economic policy during the 1920s to intensive scrutiny. In a carefully constructed monograph that follows the revisionist interpretation, Michael Hogan emphasizes points that he believes others have overlooked. Unlike those who focus on Anglo-American rivalry, he contends that a significant degree of cooperation developed in relations between the two nations. Hogan uses the cooperative theme in a more complex fashion to argue that an associational movement comparable to the one occurring within the U.S. emerged on the international level. Multinational collaboration by private groups would insure orderly economic development, thereby avoiding the chaos caused by excessive competition inherent in *laissez faire* and the political conflicts that might occur if governments directed economic activities. The growth of international cooperation, moreover, modified the traditional open door by limiting competition in world markets.

Overall the book is a satisfying one. The author has read widely in secondary literature and made use of British as well as American archival material. He presents his case in an intelligent, straightforward manner that only occasionally is marred by cumbersome passages. Furthermore, he has many interesting things to say about Anglo-American relations. While formulating cable, radio, and

oil policies, U.S. and British officials moved to compromise positions. In addition, private and central bankers in each country collaborated with one another, and British authorities eventually accepted the American position on the necessity of reducing Germany's reparations burden. Many friction points remained, however, as we shall see.

Hogan is at his strongest when discussing the international organizational revolution that took place during the postwar decade. Although nationalistic considerations remained important, a trend to multinational cooperative capitalism occurred among the industrial nations, especially Britain and America. Clearly this process altered the open door, a fact accepted by some State Department officials who maintained that nobody could expect to protect equality of opportunity indefinitely.

The most troublesome question about the book is whether the author has gone too far in insisting that cooperation was the leading feature of Anglo-American relations. By deciding not to examine tariff and commercial policy, an issue that caused much discord, Hogan selected aspects of economic relations most likely to prove his case. Furthermore, the areas (cables, radio, and petroleum) in which collaboration was greatest involved special circumstances that allowed officials to justify exceptional treatment. They may have been the exceptions rather than the rule. The informal entente, moreover, did not become apparent until the mid-1920s and broke down at the end of the decade when the first hints of trouble surfaced. One might argue that for a relationship to become an entente, even an informal one, it has to be more durable than this. Finally, as the author admits, Britain and America could not reach agreement on the allocation of resources such as rubber even when the cooperative spirit was at high tide. Nonetheless, the book makes a solid contribution to the field and raises provocative interpretative points. Interested scholars will look forward to comparing Hogan's arguments with those of Frank Costigliola and Melvin Leffler when they complete their studies.

FREDERICK C. ADAMS
Drake University

WILLIAM E. AKIN. *Technocracy and the American Dream: The Technocrat Movement, 1900-1941*. Berkeley and Los Angeles: University of California Press. 1977. Pp. xv, 227. \$9.50.

This volume is essentially an analysis of the origin and content of the ideas of the main figures who participated in the technocrat movement of the Great Depression era. Much of William E. Akin's

material is familiar. His opening chapter locates the roots of technocracy in three sources of the progressive era: scientific management, "progressive engineering," and Veblen's ideas as expressed in *The Engineers and the Price System*. Derived largely from the books of Samuel Haber and Edwin T. Layton, the chapter covers familiar ground and should have been compressed. Much of the material in the chapters describing the ideas of Walter Rautenstrauch, Howard Scott, Harold Loeb, and other technocrat publicists might similarly have been severely reduced. In Akin's detailed exposition the technocrats are revealed as unoriginal, not very prolific, and repetitive. They emerge too, in this reader's opinion, as naive and pretentious, their thought consisting mainly in clichéd criticisms of the price system and jargon-enshrouded assertions that planning by engineers should replace the marketplace. Akin's narrative is too much a literal description of technocrat writings; it suffers therefore from the repetitious and banal quality of the writings themselves.

Akin does raise and attempt to interpret a number of interesting issues in his later chapters. Technocracy appealed, he believes, employing social-psychological categories often used to describe Progressives, to middle Americans having no conscious class allegiance, biased against capitalist managers and attracted to the "harmony of interests" theory of society. They apparently were willing to accept technocracy's claim that production and distribution were best left to the "technate." What Akin does not strive to make clear is whether technocracy's followers understood Scott's anti-democratic, antiliberal views and his conclusion that, since all the important decisions should be made by his technate, politics would be abolished in his new world. Scott's societal blueprint, as Akin shows in an excellent chapter on the fully elaborated technocrat ideology of 1934-35, was chilling. A hierarchically arranged elite would govern production and distribution for a docile, conditioned public in a world from which liberal education and culture would be banished. Akin might well have emphasized more the authoritarian character of technocracy and of its key figure, Scott. He might have sought too a fuller explanation of why Scott, eccentric but persuasive, a poseur capable of commanding attention, for a time at least, from men of stronger intellect and superior training, never attempted to turn technocracy into a mass political movement, deliberately adopting an apolitical stance for his reform movement, which he thought soon would succeed on the basis of need and the sheer rationality of its premises.

ROBERT F. HIMMELBERG
Fordham University

C. L. SONNICHSEN. *Colonel Greene and the Copper Skyrocket: The Spectacular Rise and Fall of William Cornell Greene—Copper King, Cattle Baron, and Promoter Extraordinary in Mexico, the American Southwest, and the New York Financial District*. Tucson: University of Arizona Press. 1974. Pp. x, 325. Cloth \$9.50, paper \$4.95.

Not since William Saroyan used to relate an entire novel in his title has the reviewer encountered a title as comprehensive as this one. Knowing the author, one has to believe that he is pulling his public's leg. If he is dead serious, he should be read out of the profession. I prefer that he remain.

For C. L. Sonnichsen has written a solid account of a southwestern "character" who has flitted in and out of frontier and economic history. Operating largely in a period of Western history that most historians tend to consign to middle-class mediocrity, Colonel William C. Greene built a multi-million-dollar copper holding into a twentieth-century barony that threatened to dominate Arizona because of the Colonel's wide-ranging interests. He pushed through the Santa Cruz irrigation project, became an important rancher, dominated lumbering in his region, and became a factor in border diplomacy between the United States and Mexico.

By the time Greene was fifty-three, his career was over, cut short by a buggy accident. But, as the author points out, "Men like Colonel Greene do not die entirely when their physical life is over." Greene has been lamented from Cananea in northern Sonora to Wall Street in southern Manhattan, but he has found a worthy chronicler who will insure his being remembered when the talk has run out. Steeped in the history and folklore of the Southwest, Sonnichsen knows how to put a definitive story together without sacrificing interest; at the same time he can give zest and pace to a biography without sacrificing either truth or verisimilitude. In this account of Colonel Greene he has lived up to his sense of craft.

JOE B. FRANTZ
University of Texas,
Austin

STEVEN L. SCHLOSSMAN. *Love and the American Delinquent: The Theory and Practice of "Progressive" Juvenile Justice, 1825-1920*. Chicago: University of Chicago Press. 1977. Pp. xii, 303. \$15.00.

"No, this book is not about the sexual practices of juvenile delinquents," Steven L. Schlossman assures us. This inference might not be uppermost in your mind but bear with the author, for he does justify the ironic sense of his title. Love seldom touched the lives of poor children who broke the

law. Yet the men and women who organized and administered reform schools and juvenile courts endlessly proclaimed their affection for these children. Schlossman's study analyzes this gap between theory and practice. In part one, he examines doctrine, "the genesis and transformation of the 'progressive' viewpoint in nineteenth and twentieth century juvenile justice"; in part two, he skillfully utilizes institutional records and juvenile court transcripts to explain the practice of juvenile justice in Wisconsin.

Schlossman's first section explores legal interpretations of delinquency and the initial houses of refuge but focuses on the emergence of the family reform school in the 1850s and the juvenile court in the early twentieth century. These were decisive developments because, for the first time, treatment was supposed to appeal to the child's "native capacity to love and be loved as the most effective spur to moral regeneration." No longer were children to be subjected to the impersonality and workshop discipline characteristic of the refuges. In the family reform schools, socialization was to take place in cottages housing "one to three dozen inmates with similar personality traits" and presided over by a "surrogate father and mother" whose "personal assistance" comforted the child. The juvenile court extended the ideals of the family school. Henceforth, "affective discipline" would exemplify the conduct of judicial proceedings, the administration of court detention homes, and the methods of probation officers.

The author ably relates both the family schools and the juvenile court to social and cultural change. The schools, as part of a "broad reorientation in values . . . enshrining sentiment and emotion in human affairs," were connected to the domestic advice literature of Lydia Maria Child and Catharine Beecher, whose sanctification of the family "helped legitimate the family reform school and domesticate the Jacksonian house of refuge." Schlossman sees the juvenile court as systematically related to a variety of reforms, including the playground movement, and mothers' pensions and home economics programs, all of which were designed to strengthen the families of the poor and "to replace the institution as the ideal center of correction and uplift."

Schlossman contends that affectional discipline failed in practice because it was applied in a "sloppy" and "superficial" manner. Wisconsin's first "cottage" reform school (1860) floundered immediately as the bickering state legislature abandoned the cottage design for one large and cheaply constructed building. Overcrowding and periodic outbreaks of typhoid turned daily life into a struggle for survival. Inmate arsonists, including "Charles, age eight," destroyed the first institution

in 1866 but the second fared little better. Health problems, political chicanery, and escapes continued, official brutality increased, and, cottages became places to organize contract labor and to isolate troublemakers rather than homes to nurture and rehabilitate.

The Milwaukee Juvenile Court was equally unsuccessful in applying the tenets of affectional discipline. Probation and out of court settlements did lower the rate of reform school incarceration and a parental delinquency law occasionally protected children. But the detention center quickly became a children's jail while judges browbeat defendants, especially those without lawyers, and punished children for offenses unrelated to the charges that brought them into court.

Schlossman's guardedly optimistic conclusion strikes the only incongruous note. He maintains that a truly child-centered approach, epitomized by Ben Lindsey's juvenile court, was a seldom realized but attainable ideal. His principal evidence, however, has documented a policy of rhetorical affection and neglectful treatment. Reformatory and court authorities showed little interest in establishing personal relationships with those delinquent children whom they had to supervise, while they eagerly counted as "successes" those children who dropped out of sight. And these officials were not members of a status conscious local elite but often shared the disadvantaged origins of their clients. In sum, the adult personality sketched here seems fundamentally shallow and manipulative. Schlossman does not envision the emergence of an equitable policy without "broader effort of social change" but he has not demonstrated that the basis for this change existed or exists.

This comment is not intended to obscure Schlossman's achievement. He has written a thoughtful monograph which adds substantially to our understanding of the American system of juvenile justice.

ROBERT M. MENNEL
University of New Hampshire

HAROLD S. WECHSLER. *The Qualified Student: A History of Selective College Admission in America*. New York: John Wiley and Sons. 1977. Pp. xvii, 341. \$18.95.

What might have been a desiccated record of American college and university admissions practices here becomes a significant social history of innovative policies at the University of Michigan, Columbia University, the University of Chicago, and the City University of New York. Harold Wechsler's central theme is the complex and inevi-

table relationships among admissions policies, institutional goals, and general social conditions. Universities are not only shaped by society; they also affect that society by recruiting for the professions through their admissions, thus affecting the nature of social mobility. To illustrate this theme, Wechsler devotes half of his book to Columbia's changing admissions policies. The result is the best critical appraisal so far of Nicholas Murray Butler's long tenure as briefly a dean and then president at Columbia (1890-1945).

Almost everyone knows that "Nicholas Miraculous" brought Columbia to academic greatness by 1910. What Wechsler strikingly reveals, however, is the social bankruptcy after the Progressive era of Butler's concept of a deferential democracy exclusively led by white Protestant college alumni. Wechsler argues that the social anti-Semitism of Butler, of leading Columbia trustees, and of admissions officers after 1920 reversed the admissions policy these men had earlier relegated to the high schools and later to the standardized admissions examinations of the College Entrance Examination Board. By 1910 about half of all New York City high school students were of Eastern European Jewish background. Butler had not foreseen that the high schools would become vehicles for upwardly mobile but to him socially undesirable Jews. Driven by fear of losing Columbia's "natural" old-stock constituency, Butler and his deans after 1920 employed college entrance examinations and detailed application forms to return the screening process to the selective control of the college's admissions officers. Rigorous selectivity made a *numerus clausus* unnecessary, but even that was used in the 1930s while Butler encouraged his admissions dean to stress geographical representation, "family background, character and personality," and not to depend "exclusively upon the results of formal examination tests." No longer the Butler of 1900! Closely related here are the discriminatory admissions policy of Columbia's Medical School after 1918 and the University's trustees' even longer unwillingness to invite a Jew to the Board until Benjamin Cardozo's election in 1928 followed by Arthur H. Sulzberger's in 1944—this despite Jacob Schiff's munificence toward Barnard and Teachers Colleges in the 1890s.

All this is an infamous and sorry tale. It needs telling. I should have preferred to see the book limited to Butler and Columbia. There is a certain imbalance in the addition of a chapter on the University of Chicago's problems after the 1920s when it sought new students imbued with the "university spirit" of high academic achievement. The final three chapters trace the transition from "elite" to mass higher education since the twenties. They are as fresh as today's headlines

about university admissions cases appealed to the courts. In a lengthy chapter that chronicles events leading to "open admissions" of all high school graduates to the City University of New York in 1970, Wechsler emphasizes the admirable perseverance of Chancellor Albert Bowker. Despite its subtitle, Wechsler's book is not comprehensive or definitive, and several "case studies" only detract from the central argument about Columbia. Nevertheless, if the reader ignores the idiotic chapter subtitles, he or she will find here penetrating social history and, even more, pricking and recurring questions well posed about access to higher education in our society.

WILSON SMITH
University of California,
Davis

PAUL A. CARTER. *Another Part of the Twenties*. New York: Columbia University Press. 1977. Pp. xiii, 229. \$9.95.

Ten years ago Paul Carter wrote one of the more interesting short books about what seems to be a perpetually fascinating period: the 1920s. That volume (*The Twenties in America*) summarized the topics ordinarily found in books on the era, with especially perceptive sections devoted to prohibition and fundamentalism. Now Carter claims that the traditional emphases in works on the 20s require correction. His own father, he discovered, a student at an eastern men's liberal arts college when F. Scott Fitzgerald's novel about student life, *This Side of Paradise*, was published, never read the book. Nor did he own a raccoon coat. Carter's mother, a "strikingly attractive, high spirited" college-educated woman, preferred church socials to the kinds of fun usually associated with the flappers of the 20s. Carter reports that when his recent students asked relatives who had lived as adults during the 1920s to describe their lives at that time he received the impression that most respondents, like his parents, missed most of what we characterize as the "jazz age" or the "roaring twenties." Therefore, we are led to believe at the outset of this book, Carter decided to write about these ostensibly more representative experiences and perceptions of the period: *Another Part of the Twenties*.

The fundamental differences between his work and more conventional treatments, he implies, lie in his stress on positive aspects of the era, and on the predominantly rural nature of the country. Presumably these emphases are connected, though the author never makes the linkages clear. The overall effect of Carter's orientation is to diminish somewhat the widespread view that after World War I the outlines of modern America were visible.

It is difficult to tell, however, whether his intentions are that broad or profound.

Carter is an engaging writer, with an eye for the apt quotation and an obvious relish for provocative statements. Most readers familiar with the period will enjoy reading this book. Nevertheless, in straining to be different, and to tie problems of the twenties to those of today, Carter presents some interpretations and examples that are glib and occasionally absurd. Certain of his comments about Einstein, Freud, Horatio Alger, feminism, and the presidential election of 1928, for example, will distress specialists in those subjects.

Scholars led by Carter to expect important new interpretations, or even those hoping to discover a distinct "other side" of the 1920's will be disappointed. In a diffuse first chapter containing many interesting comments about small-town values and life styles, Carter makes a fairly persuasive case for the proposition that these were more prevalent than most other historians of the 20s have realized, and he suggests that this heritage remains underestimated by historians of more recent periods. A later chapter discusses the burgeoning advertising industry. It may be the most illuminating essay available about the preparation and content of ads in the 20s. As Carter observes, "for a person raised in modern mass society these advertisements have a haunting familiarity." He certainly takes a positive view of American advertising; at times his praise resembles tracts published by advertisers' associations or by J. Walter Thompson, Inc. The relationship of these modern ads to the failure of Carter's father to buy a raccoon coat or to his mother's preference for church socials is not readily apparent, however. Nor do the ads seem to reflect rural values or life-styles.

As in Carter's earlier book on the 1920s, the truly creative and sometimes strikingly original sections of this volume deal with the subtleties of Protestant religious beliefs, especially within evangelical sects, and with prohibition and prohibitionists. In discussing these closely connected sectors of American history Carter demonstrates a rare degree of insight and empathy. Yet he fails to connect those sections to his more conventional chapters on pacifism, presidential politics, advertising, or even rural life. Carter obviously possesses the unique knowledge and ability needed for a book which would justify his title, but this volume of basically unconnected essays is not such a book.

STANLEY COBEN
*University of California,
Los Angeles*

KENNETH R. PHILP. *John Collier's Crusade for Indian Reform, 1920-1954*. Tucson: University of Arizona Press. 1977. Pp. xvi, 304. Cloth \$12.50, paper \$6.50.

This first Collier biography deals primarily with his career as leader of the new reformers in Indian affairs in the 1920s and as Indian Commissioner under FDR. His activities before 1920 and after resigning the commissionership in 1945 are outlined briefly.

In 1920, Collier discovered what Kenneth R. Philp calls a "Red Atlantis" at Taos Pueblo. It was so powerful an experience that he thereafter cast the other tribes in the image of the Pueblos, while assigning all Indians a redemptive relationship to white society. (The red man's burden?) Pursuing this vision, Collier brought into the 20s battles for Indian rights influential groups that shared his cultural pluralist views, in the process often opposing the gradualist melting-pot approach of the older reformers led by the Indian Rights Association.

Philp rightly characterizes this period as "a seedtime for Indian reform." Its first fruits were the new programs instituted by Hoover's reform commissioner, Charles Rhoads, former president of the IRA. Collier initially welcomed, but soon bitterly attacked, the new Indian administration. Philp believes that "the basic changes required for a new Indian policy had been formulated" by 1933.

When Collier in his turn became a reform commissioner under Roosevelt, he set about translating his own vision into practice. Philp demonstrates the frustrations involved in even the most devoted efforts at quick or permanent "solutions" to "the Indian problem." Collier's primary legislative creation was the Wheeler-Howard bill which eventually became the Indian Reorganization Act of 1934. It was the most fundamental piece of Indian legislation since the 1887 Dawes Act. Philp describes its genesis, its transformations during the political infighting before enactment, and the differences among Indians over its provisions. In its final form the Act was an almost completely redrafted compromise measure. Philp shows how Collier used the executive powers of his office to bring its operation as close to his original ideas as he could, and describes the conflicts in the tribes over adopting the constitutions and charters it authorized.

Philp's book is a useful addition to the growing literature in twentieth-century American Indian history. But, it has severe limitations. It is narrowly political. The historical context is dim and the social analysis minimal. Collier himself and the people who worked with him and against him do not come alive. His relations with other reformers are inadequately treated. Collier's American Indian Defense Association, which he created as his organizational vehicle, remains shadowy, perhaps because the author relied on published AIDA

literature and Collier's papers, and did not examine the AIDA archives.

Thus the book is by no means definitive as a biography, or as an analysis of the seedtime of the 20s, or as an account of the Indian New Deal. However, Philp's work should encourage further exploration of a neglected aspect of American history, and specifically of the life of a charismatic man who, to an extraordinary degree, reshaped public opinion and public policy in the lineaments of his personal vision.

HAZEL WHITMAN HERTZBERG
Columbia University,
Teachers College

NEIL BETTEN. *Catholic Activism and the Industrial Worker*. Gainesville: University Presses of Florida. 1976. Pp. x, 191. \$10.00.

Despite the large number of Catholics who have actively participated in the American labor movement, both as leaders and as rank-and-file members, very little has been written on the significant role played by Catholics in the American worker's long struggle for recognition and economic justice. This small, carefully researched volume by Neil Betten is a praiseworthy attempt partially to fill this glaring void in American labor history.

Betten acknowledges that American Catholic leaders were far ahead of their time in their open support of labor unions in the late nineteenth century, but contends that they had become very complacent by the 1920s. The Great Depression jarred them out of their complacency, as it did so many other Americans. Catholic leaders responded vigorously to the economic crisis of the 1930s and, for the first time, supported truly radical approaches to social justice. Most went so far as to support the Communist-tainted CIO in its battle with the AFL to organize the long neglected industrial workers. As the ideological tides shifted in the late 1940s, Catholics led the way in expelling the Communists from organized labor. Betten contends that this upsurge of anti-Communist conservatism not only destroyed Communist influence in the CIO as intended, but also "buried the American Catholic social and economic goals of the 1930s, for these seemed too radical and therefore un-American" (p. 150). This is plausible enough up to a point, but others might argue that militant reform was largely a victim of its own success. Economic conditions in the 1940s had greatly improved for the vast majority of American workers. There was far less need for either radical rhetoric or action.

Of special interest to this reviewer was the very informative chapter on the labor activities of Father Charles E. Coughlin, who is, of course, much

better known for his radio broadcasts and political activities. The book also contains short summaries of the labor involvement of such diverse personalities as Dorothy Day and Peter Maurin of the Catholic Worker movement, John A. Ryan, director of the Social Action Department of the National Catholic Welfare Conference, Father Charles Owen Rice of Pittsburgh, and Father John P. Boland of Buffalo.

Betten presents little that is really new to anyone reasonably well read in American Catholic history. A notable exception is his chapter on Father Coughlin, which is the only full-scale treatment of Coughlin's labor activities yet to appear in print. Though most of the material is available in piecemeal form elsewhere, Betten has performed a very real service by making this material readily accessible in one convenient volume. With the current revival of interest in ethnic and religious studies, it is to be hoped that detailed studies will be made in the near future of many of the men and organizations that the author was forced to treat in so abbreviated a manner. The author makes no claim to have written the definitive history of Catholics and American labor, but his book is a very solid beginning to the long overdue scholarly treatment of this important subject.

CHARLES TULL
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South Bend

ELLIOT A. ROSEN. *Hoover, Roosevelt, and the Brains Trust: From Depression to New Deal*. New York: Columbia University Press. 1977. Pp. x, 446. \$16.95.

This major work, primarily covering the year 1932, is based in part on research in some fifty manuscript collections. It challenges several established interpretations in a lively and persuasive manner. In comparing the responses of Hoover and Roosevelt to economic collapse, Elliot A. Rosen also convincingly questions the recent rehabilitation of the Great Engineer (discussed by Robert H. Zieger in the *AHR*, October 1976). Historians who have characterized Hoover—and the labels are legion—will have to contend with Rosen, who would agree with Robert F. Himmelberg's assertion that Hoover's commitment to corporate associationism was always limited by his fidelity to the ideal of competition as the organizing principle of the economy. Commenting on Hoover's *American Individualism* (1922), Rosen states, "Despite Hoover's contention that his interpretation of individualism posited an abandonment of eighteenth-century laissez faire attitudes, it represented in reality the enshrinement of nineteenth-century Social Darwinism meliorated by certain civilized ground rules under the umbrella of voluntarism, or com-

petition within structured interest groups." The kind of cooperation Hoover espoused was, Rosen writes, only "superficially innovative, actually a recognition of what had already developed in American life," although Hoover was naive in his expectation of public-spirited behavior on the part of private groups. In sum, scratch an advocate of will-o'-the-wisp cooperationism, which in the real world amounted to concentration without control, and you will find a Darwinian individualist underneath.

Hoover's and Ogden Mills' disastrous policies of fiscal orthodoxy and a quest for restoration of the international gold standard, Rosen maintains, reflected underestimation of the depth of the Depression and failure to comprehend its basic sources. Meanwhile, in 1932 Roosevelt and the Brains Trusters—Berle, Moley, and Tugwell—synthesized a viewpoint resting on recognition of the profundity and complexity of the causes of collapse, rejection of dogma, and insistence on an enlarged federal role to counter concentrated private power while fostering internal economic balance and, through stimulative security measures, enhanced social justice. Thus, to call Hoover the father of the New Deal is to obscure his ideology and policies. Rosen's reconstruction of the exchange between Roosevelt and the Brains Trusters, moreover, undermines the widely held view that the New Deal was essentially an improvisation as well as the "Two New Deals" interpretation. Nor will William Appleman Williams, a key figure in the rehabilitation of Hoover, and his disciples find confirmation in this book. Not only does Rosen deprecate Hoover's voluntary cooperationism, which Williams lauds while neglecting Hoover's competitive individualism, but he also denies Williams' thesis that our interwar diplomacy derived from a search for overseas markets. "Intranationalism" or reasonable self-sufficiency guided New Deal foreign policy.

Finally, Rosen is enlightening on politics. He demythologizes Jim Farley and Louis Howe, finding them far from sophisticated as they made their way among rival Democratic factions. In fact, they came close to losing the nomination to a powerful conservative coalition, a group which in the mid-thirties joined with Republicans to bring the New Deal to a halt. The intellectual roots of this bipartisan coalition, Rosen says, lay in Hoover's individualism, an expression of "the nation's traditionalist consensus." One suspects that as Rosen pursues the politics of the late thirties and beyond in detail and considers studies by James Burns, Charles Gray, John Manly, John Moore, James Patterson, Wayne Shannon, and others, he will provide a richer, multifaceted analysis of the bipartisan congressional coalition.

Historians of the Depression and the New Deal who dispute Rosen's conclusions will encounter a formidable adversary. All will find this work, along with a half dozen others, indispensable.

BERNARD STERNESHER
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DAVID MACISAAC. *Strategic Bombing in World War Two: The Story of the United States Strategic Bombing Survey*. New York: Garland Publishing. 1976. Pp. xi, 231. \$15.00.

For nearly five years during World War II the Allies engaged in a strategic bombing offensive against the enemy; three decades later the effectiveness of that massive effort remains controversial. This long overdue study attempts to resolve part of that controversy, namely the role of the United States Strategic Bombing Survey (USSBS), which, as the war wound down, undertook to measure the success of American Air Force daylight, high altitude, precision bombing.

David MacIsaac has written a factual account of the origins, objectives, personnel, and methods of the Survey; thus he lays to rest some questions long debated. For example, the idea for USSBS, headed by a civilian but containing both military and civilian experts, originated with the AAF. Its purpose was two-fold: to measure AAF effectiveness (and hopefully to justify faith in its strategic philosophy) and to assure its postwar future. Also, the Survey's objective was to ascertain the effect of strategic air bombardment on the war's outcome, not to render moral judgments on the use of air power, or to settle questions of the relative importance to victory of the services. Although the final report was partial to the air weapon, USSBS qualified its endorsement of strategic bombardment by frequent references to the role of "Allied air power." On the other hand, its conclusion that the use of atomic weapons against an already defeated Japan was probably unnecessary naturally heated up that ongoing debate.

In evaluating the Survey's work, MacIsaac chose not to fish in the troubled waters of the relative importance of a strategic air force; instead, his judgments were confined to the Survey itself. Thus he concludes that, under the restrictions of its limited objectives, time, and its bureaucratic makeup, USSBS performed its task well. Its findings were not widely used, however, partly because of the belief that nuclear weapons would change the nature of warfare. Korea and Vietnam, of course, proved otherwise. And, as far as the Survey's top-level management was concerned, a major but hardly surprising weakness was the absence of any historian "noted for his ability to ask tough questions of the evidence" (p. 160).

To MacIsaac, the Survey's findings (published under his editorship in ten volumes in 1976) constitute the "Domesday Book on strategic air warfare in World War II." As such, those findings are rendered more useful by this brief, authoritative account of the Survey and its efforts.

C. L. GRANT
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MICHAEL S. SHERRY. *Preparing for the Next War: American Plans for Postwar Defense, 1941-45*. (Yale Historical Publications. Miscellany, number 114.) New Haven: Yale University Press. 1977. Pp. x, 260. \$12.50.

From the moment of American entry into World War II, the armed services began planning for their postwar defense policies. Each service had its own parochial interest—the Navy wanted to assure itself of a continued position as the first line of defense, and thus as the most important service; the Army Air Force wanted independence for itself and a national commitment to strategic airpower as the first line of defense; the Army desperately wished to avoid a repeat of the policies of the twenties and thirties, when the Army had been sadly neglected. Given their different aims, there was necessarily a great deal of tension between the services over what the defense policy of the nation should be. Previous studies of the Navy's and the Air Force's postwar planning have emphasized this tension.

It is one of the many virtues of Michael Sherry's study that he steps back from the parochial viewpoint to see the picture whole. While not ignoring the in-fighting, he concentrates on the points the services agreed upon, on their common assumptions and goals. He finds a surprisingly large area of agreement, most of all on unstated assumptions about America's role in the postwar world, and on the nature of that world. Obviously, such assumptions were the major factor in making up plans about the defense of the U.S.

A number of points stand out. First, the experience with the League of Nations made the planners certain that the new United Nations could not be looked to or counted on to keep the peace. Second, they were certain that the "next war" would resemble the one that was currently being fought; as Sherry puts it, "Engaged in one holocaust, they concentrated on planning for another." Next, Sherry emphasizes that the planners did not regard Russia as the most likely future enemy; rather, they believed that "the primary danger to American security arose from fundamental changes in the conduct of war and international relations, not from the transient threat posed by a

particular nation." And if technology was the enemy, then the most dangerous threat came from a revived Germany and Japan, not backward Russia.

In some ways, war itself was the great danger. Here, of course, the professional military were merely following the lead of their political masters, most importantly President Franklin D. Roosevelt, who insisted that the U.S. was fighting in World War II against not just Germany and Italy and Japan, but against war itself. Since this view provided a perfect rationale for an expanded defense establishment after the war, the services naturally enough made "recommendations on deterrence and preventive war that constituted a brief for the U.S. to become the world's policeman and peacemaker." As Army Chief of Staff George C. Marshall explained in his final report, "We are now concerned with the peace of the entire world."

The irony is that by adopting such a messianic position, Marshall was undercutting the Army's proposed program of Universal Military Training, because it was impossible to convince Congress that a massive reserve force could deter war as effectively as a powerful Air Force. The preparedness ideology inevitably led to a greatly expanded Air Force, at the expense of the Army and the battleship Navy. The Air Force also received support from various "interlocking interests . . . and not merely the vested interests which faced severe contraction at war's end." The ideology "served the interests of the capitalist economy as a whole" because "preparedness would help nourish the scientific infrastructure needed for economic expansion, strengthen ties between the corporate and political elites, and defend access to the markets and materials deemed necessary for continued economic growth. And preparedness could stimulate gross investment at a time when other forms of government spending were anathema to powerful interests" (p. 236). These factors, Sherry correctly points out, were relatively unimportant to the military planners, but "they enhanced the appeal and the utility of the preparedness program, and its acceptance as the cold war went on."

Sherry has an excellent chapter on the marriage between university scientists and the military, and another on Universal Military Training. He is critical of the planners for failing to see that anti-colonial wars, not more global holocausts, were the wave of the future, but such criticism seems misdirected—why should the professional military be any smarter than others, including politicians, responsible for planning for the future? Overall, this is a first-class, original study of a neglected but important subject.

STEPHEN E. AMBROSE
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DANIEL YERGIN. *Shattered Peace: The Origins of the Cold War and the National Security State*. Boston: Houghton Mifflin Company. 1977. Pp. x, 526. \$15.00.

Daniel Yergin has written an important book. Tracing the origins of the Cold War from the Yalta Conference to the Berlin blockade, he argues that the primary cause was the failure of American diplomats to understand the nature of the Soviet challenge. Franklin Roosevelt, whom Yergin aptly describes as a renegade Wilsonian, sought a great power peace. FDR's "Yalta axioms" were designed to concede Eastern Europe to the Russians as their legitimate sphere of influence, thereby confirming the 1944 Churchill-Stalin arrangement. His death in 1945, however, opened the way for the opposing "Riga axioms." Advanced by Soviet experts in the State Department, notably George Kennan, this rigid set of beliefs that the Soviet Union was a revolutionary state bent on world domination quickly prevailed in the Truman Administration. Compromise with the Kremlin was viewed as impossible and power replaced diplomacy as the prime consideration in American foreign policy. Secretary of State James Byrnes tried to keep the Yalta axioms alive, but by early 1946 a reliance on military strength led to the creation of "the national security state" and insured at least three decades of unending tension.

Yergin's book is by far the most sophisticated and convincing revisionist account of how the Cold War began. Unlike many earlier critics of Truman's foreign policy, he is a master of the historian's craft—probing deeply into a wide variety of manuscript sources, utilizing the existing literature, regardless of viewpoint, fairly and intelligently, and writing superbly. Even more impressive is his judicious treatment of the evidence. Avoiding the polemics which mar so many revisionist accounts, notably those of Gabriel Kolko and Gar Alperovitz, he states his views reasonably and persuasively. Thus, while critical of the Truman Doctrine, he perceives it as an honest statement, one presenting publicly the same ideas and arguments Truman's advisers used in their private deliberations. Similarly, he sees the Marshall Plan as aimed both at halting Soviet expansion and revitalizing Western Europe, but not as a conscious effort to assert American economic imperialism. Even when this reviewer disagreed with Yergin, he felt that he was engaging in a dialogue, not a personal confrontation.

Two flaws weaken Yergin's analysis. The first is his elitist approach. The guiding assumption of his study is that the president and his advisers determine American foreign policy with little regard for public opinion. Accordingly, he focuses on Roose-

velt and Truman, on Hopkins and Harriman, on Kennan and Forrestal, relying on their public records and private papers to reconstruct the course of American diplomacy. He rarely cites congressional documents, newspaper editorials, or public opinion polls, and when he does, it is mainly to argue that the public attitude toward Russia was contradictory and confused, and thus permitted the administration to do as it pleased. The general public exerted no influence on the policy-makers; the people simply responded to the clues from above. Yet Yergin's own interpretation of Roosevelt's Yalta axioms denies this simplistic notion. FDR's success, he argues, lay in secretly pursuing a great power peace while catering to the public's desire for an idealistic international order founded on the United Nations. The Cold War was as much the result of public disillusionment with Roosevelt's devious tactics as it was of Truman's tendency to abandon the sphere-of-influence arrangement.

Even more disturbing is Yergin's ethnocentric approach. Focusing his attention on American policy-making, he assumes that the Soviet Union always acted in response to American initiatives. Truman's attempts to implement the Yalta accords in Eastern Europe led to the creation of the satellite empire, the extreme rhetoric of the Truman Doctrine caused the Council of Foreign Ministers deliberations to fail, and the London agreement to sponsor a West German government forced Stalin to resort to the blockade of Berlin. Throughout, we are told that Stalin was cautious and prudent in his foreign policy, always willing to engage in the give and take of diplomacy. Americans, on the other hand, were rigid and inflexible, prisoners of the Riga axioms and their Wilsonian heritage. The Cold War is thus the result of a diplomatic failure of the United States, a nation governed by ideology, and not of the Soviet Union, a nation guided by realism. The Russians, deprived by Yergin of an independent source of behavior, simply responded to American actions, and by this curious double standard, they are absolved of any moral responsibility. Yes, Stalin was a ruthless man, Yergin tells us; yes, the Soviets imposed their brutal will on Eastern Europe; yes, the Czech coup was a tragic event—but the United States is responsible for all these unfortunate developments because its aggressive policies forced the Soviets to act as they did.

Despite these flaws, *Shattered Peace* should become the basis for all future scholarly debate on the origins of the Cold War. Yergin has settled many of the old issues and raised new ones about the nature and direction of American foreign policy after 1945. In particular, his contention that the Cold War transformed concern about national de-

fense into a global obsession with national security merits careful consideration. He has broken down the artificial barrier between diplomatic and strategic policy, and future historians will have to follow his lead in examining the interaction between the State Department and the Pentagon in accounting for the Cold War policies of the United States. One hopes they will not ignore, as Yergin does, the equally important roles played by both American public opinion and the men in the Kremlin, who acted out of the imperatives of a revolutionary ideology and a distinctive historical experience.

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GEORGE T. MAZUZAN. *Warren R. Austin at the U.N., 1946-1953*. Kent: Kent State University Press. 1977. Pp. xi, 233. \$10.00.

Warren R. Austin served for seven years as the first United States Ambassador to the newly formed United Nations. Few people then or now remember much about Austin, and his career is principally interesting for the ground that it traversed.

George T. Mazuzan has done a competent job of marking the almost classic evolution of Austin's career from anti-New Deal senator from Vermont to Republican internationalist on the eve of World War II; from fervent believer in the ideal of the United Nations to mouthpiece for the Truman Administration's Cold War policies. It was a road taken by no small number of other Republicans, and one which deserves the attention Mazuzan has given it. Had the Republican Party's belated conversion to internationalism not become simply a more shrill and obsessed version of anti-Communism than that of the Democrats it might have made a more constructive contribution to postwar American foreign policy. But, as it was, Republicans like Austin were too much the prisoners of their parochial and legalistic backgrounds to have anything to contribute to an America increasingly caught up in the rip-tides of power politics. They were like pieces of lumber swept along by powerful currents, never able to establish a mooring that might have diverted postwar foreign policy away from the Cold War channel.

In the Senate from 1928 to 1945 Austin was noteworthy as a follower within the Republican establishment. Only the working out of his patriotic belief that America had a mission to lead the world along democratic and capitalist lines led him to deviate from the isolationism dominant within his

party. Throughout his transformation he was inspired by the idea that the United Nations was "the perfect symbol of a nation emerging from isolation and that America must channel its leadership through that world body." Anyone as blinkered as that was doomed to play a futile role when it became evident that America was not going to channel its foreign policy through the U.N.

Austin's early support for the United Nations won him Truman's nomination as chief of the U.S. delegation. A legalist to the core, Austin had no sense of the forces animating other nations or of the motives of his own government. As a consequence he was totally out of his element when American policy shifted from universalism to fighting the Cold War. Like Vandenberg he rationalized the tactics of containment and alliance as a prop for the United Nations until that body could develop its effectiveness. Austin blindly moved from devotion to the U.N. to being a mouthpiece for his government's Cold War policies, seemingly never aware of how far off course he was.

Mazuzan's study provides a perfect demonstration of the fate that was to befall a succession of U.S. ambassadors to the U.N. The Truman Doctrine marked the abandonment of universalism in favor of a strategy of containment. No one in Washington consulted Austin about the formulation of that policy. Few U.N. ambassadors have been as ignored and used as Austin was, but few have escaped his fate as the second volume of John Bartlow Martin's biography of Adlai Stevenson makes plain. Nevertheless the sincerity (naiveté) of Austin's beliefs permitted him to argue America's cause in the U.N. with a conviction which in a more cynical person might not have been possible.

Mazuzan concludes that much of Austin's value lay in his function as a placebo. Having oversold the American people on the U.N., policy-makers could never openly abandon it. Much of Austin's value to the Truman Administration lay "in the manner in which he continued to uphold America's belief in the United Nations despite a policy that moved away from major use of the world body to conduct foreign relations." Mazuzan has written a clear and readable account of Austin's career. He has successfully shown how the limitations of the legalistic mind, and of the believers in America's essential virtue—among whom we might also count Robert Taft and Vandenberg—may have failed to provide American foreign policy with a creative opposition at a time when Truman and Acheson were moving more and more toward a Cold War stance both abroad and at home.

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SAMUEL L. BAILY. *The United States and the Development of South America, 1945-1975*. New York: New Viewpoints. 1976. Pp. ix, 246. \$15.00.

My chief criticism of this generally useful book is that Samuel L. Baily has fallen between the two stools of narrative and analysis, and has therefore not quite done justice to either. Chapter one presents a rather hasty analysis of "diffusionist" versus "dependency" theories of Latin American economic development, in which dependency theory in particular fails to receive its due. Chapters two through four provide a rapid survey of U.S. Latin American policy from the beginnings to 1975 without offering very much new material. Baily is at his best in chapters five and six, largely because he has delimited his subject matter more carefully and has therefore given each of his themes (the post-1964 U.S.-Brazil partnership and U.S. policy toward Allende) the attention it deserves. In his seventh and concluding chapter, he offers some sound policy advice, but his recommendations are limited by his incomplete analytical framework.

Specifically, the key contention in Baily's discussion of dependency theory is that it is "more a critique of diffusion than a conceptualization of development" (p. 12). He finds it of limited value because it "offers us little specific guidance on how the currently underdeveloped countries can develop" (*ibid.*). That is not the function of dependency theory, however. Its purpose is to expose the counter-productiveness of existing U.S.-Latin American relationships so as to clear the track for social experimentation by the Latin Americans themselves.

Baily's incomplete analysis of dependency theory thus raises questions about his concluding policy prescriptions. I cannot argue with his proposals to terminate both U.S. bilateral aid programs (military and economic) and U.S. government involvement in investment disputes. However, I do question his suggestions that such economic aid be replaced by increased multilateral aid and that U.S. technology be made "more readily available" (p. 224). Dependency theorists such as Ivan Illich have shown that technological importation can create more dependency and underdevelopment than it eliminates (Baily mentions this point briefly on page 10 but does not pursue it). The notion that multilateral organizations are politically independent of the U.S. government has also been widely attacked. Nor are such organizations necessarily more efficient in their efforts at promoting development (cf. William and Elizabeth Paddock's excellent study, *We Don't Know How* [1973], a devastating critique of both bilateral and multilateral aid programs).

In short, one wonders if Baily's recommendations would have been the same had he followed through more fully in his analysis of dependency theory. It may not be intellectually satisfying to present merely negative policy proposals, but those may nonetheless be the most valid proposals which U.S. historians can make in this case.

DAVID GREEN

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Foreign Relations of the United States, 1949. Volume 7, *The Far East and Australasia*, part 2. (Department of State Publication 8857.) Washington, D.C.: Government Printing Office. 1976. Pp. x, 601-1,220, xxi. \$9.30.

This volume in the *Foreign Relations* series is concerned with Japan and Korea and includes a section on proposals for Asian regional associations or pacts. Not all documents of the period are printed, but rather only those cleared by the National Security Council, the Department of Defense, the Central Intelligence Agency, and the State Department. Even with these restrictions, a significant number of documents were declassified, and they provide a clearer picture of American diplomacy in Asia in 1949. As usual, the volume is well indexed and has excellent cross-reference footnotes.

Although several serious problems are analyzed in the 234 pages of documents on Japan—including the reparations issue, peace treaty proposals, purges, and economic and defense questions—the most significant historical sections deal with the State Department's evaluation of the Occupation. By 1949, State was extremely critical of the Occupation. It strongly advised the curtailment of Occupation controls and recommended that the Supreme Commander of the Allied Powers (SCAP), General Douglas MacArthur, be restricted to purely military duties. According to W. Walton Butterworth, Director of the Office of Far Eastern Affairs, the Occupation "began to approach the point of diminishing returns" as early as July 1947 (p. 752). Max W. Bishop, Chief of the Division of Northeast Asian Affairs, declared: "Even General MacArthur has lost his aura of sanctity" (p. 663). William J. Sebald, political adviser in Japan and an astute observer, concisely presented State's position: "The Occupation will leave deep scars . . . , scars which cannot be healed by an undue and indefinite prolongation of the dictatorial, often tactless, and sometimes slipshod methods of a military regime" (p. 832). State believed that SCAP

should "shift responsibility as rapidly as possible to the Japanese, with a corresponding reduction in SCAP personnel, that the Japanese should be permitted to proceed in their own way with assimilation of the reform programs, and that the psychological impact of the occupation on the Japanese should be reduced to a minimum" (p. 753). State also strongly recommended the appointment of an ambassador who would be in charge of all civilian matters. Members of State recognized that it was probably impossible to make these changes "as long as General MacArthur remains in Japan" (p. 677) and suggested that the general be ordered to Washington and replaced by Major General Maxwell Taylor.

The major issue discussed in the 234 pages of documents on Korea was the exact timing of the withdrawal of American occupation forces. The roles of the State and Defense Departments were reversed. In Korea, State advocated continued occupation until the Republic of Korea was certain of survival while the Defense Department demanded total withdrawal as quickly as possible. State's evaluation was that the "predominant aim of Soviet policy" in Korea was to achieve Soviet domination of the entire country (p. 974). The North Korean government was viewed as an "abject tool" of its "Soviet Masters" and, thus, a North Korean attack was possible. Russia, the State Department asserted, although preferring to accomplish its "objective without bloodshed," was quite willing to "plunge Korea into the abyss of civil war" (p. 962). The Army did consider the prospect of a "Full Scale Invasion" by North Korea "Subsequent to Withdrawal of United States Troops" (pp. 1046-57) and recommended that if an invasion occurred the matter be turned over to the United Nations. The position of the Joint Chiefs of Staff was clear: Korea was "of little strategic value to the United States and that any commitment to . . . use . . . military force in Korea would be ill advised" (p. 1056). Washington decided on withdrawal of all American troops by June 30, 1949. As late as May 17 the United States had not informed President Syngman Rhee although rumors were rampant. Rhee attempted to obtain a firm military commitment, but Washington firmly, almost harshly, refused to contemplate any bilateral or multilateral military pact with South Korea.

The concluding section of this volume deals with proposals for regional associations in Asia. There were many attempts by Asian leaders, particularly Chiang K'ai-shek, to persuade the United States to participate in a military and economic alliance referred to as the Pacific Pact. The Pacific Pact was visualized by its supporters as a coalition of anti-Communist governments including Australia,

South Korea, the Philippines, Thailand, New Zealand, and even India. Washington refused to take an active role in any regional organization. Secretary of State Dean Acheson declared: "The United States is not currently considering participation in any . . . defense arrangements" in Asia (p. 1143).

KENNETH RAY YOUNG

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HUMBERT S. NELLI. *The Business of Crime: Italians and Syndicate Crime in the United States*. New York: Oxford University Press. 1976. Pp. xiii, 314. \$12.95.

Organized crime has become a major cultural happening as well as a serious law-enforcement problem in American society. A number of recent books and films punctuated by actual gangland killings have reawakened the popular view of organized crime as a romantic, if deadly, ghetto Western. But organized crime is more than that; it is an integral part of American life, one of the many social ills—poverty, street crime, racism—which seem to come with urban living. As a result, organized crime manages to continue as an American way of life, and seems to transcend changing definitions of what is legal and what is not. Nor is there any agreement among government officials, journalists, and scholars who have studied organized crime on its definition and origins. Most government officials and the press continue to view organized crime as an alien import, one of the problems brought to America by Italian, and especially Sicilian, immigrants in the form of *Mafia*. Recently, however, a number of social scientists have described organized crime as part of the social, political, and economic character of American cities. Some subscribe to a theory of ethnic succession: first the Irish, then the Jews, followed by the Italians, and now blacks and Hispanics have used organized crime as a readily available, if perilous, route out of the poverty and powerlessness of the slum. Others, particularly economists, have described organized crime as one end of a continuum which has so-called legal business at one end and illicit enterprise or organized crime at the other.

Humbert Nelli has written a definitive history of organized crime in America without embracing or ignoring any one of the current definitions. He presents a clear and objective picture of the role of Italians in organized crime in America, returning to the Italian *Mezzogiorno* to find some of the cultural characteristics of the Italian involvement, and to nineteenth-century New Orleans and the lynching of eleven reputed *Mafiosi* for the origins of the conspiracy theory. He leads us back to the social and political roots of Italian-American in-

volverment in organized crime in the immigrant colonies and their interrelationship with American politics and society. Here, his chapter on the Black Hand for the first time demonstrates how this phenomenon of the Italian ghetto led Italians naturally, if disastrously, to the movement into American syndicate crime. In the third and final section of the book, Nelli describes the role of Prohibition and the Great Depression in the emergence of the powerful Italian-American "crime families" and their symbiosis with American politics and business.

How one defines organized crime is of more than passing academic interest. If organized crime is indeed an integral part of American life, government and public must make a concerted effort to provide viable alternatives to criminal behavior through better life chances, the decriminalization of some "vices," the elimination of corrupt practices in both the private and official sectors, and perhaps even physically reducing the opportunities for running syndicated crime operations. Nelli provides us with some compelling and convincing evidence that we must lay aside the mythology and begin to deal with the realities of the situation: organized crime would not survive without corruption in government and in business, nor would it thrive without public support.

FRANCIS A. J. IANNI
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DANIEL SNOWMAN. *Britain and America: An Interpretation of Their Culture, 1945-1975*. New York: New York University Press. 1977. Pp. 342. \$13.50.

Britain and America have generated well-defined and influential cultures, each closely related to the other. Since 1945 both cultures have experienced internal value shifts; a new balance of cultural trade has made Britain increasingly the importer. Such are the major assumptions on which Daniel Snowman bases his attack on a subject made unusually difficult not only by the intricacies of Anglo-U.S. contacts but also by the burden of defining cultural values through a range of materials extending from Puritan sermons through superhighway roadsigns.

Snowman's most obvious problems arise from his elliptic derivation of historical values. For example, he makes no real effort to distinguish between what he calls the "Puritan ethic" of the seventeenth century and that gospel of work which arose in the later nineteenth century. He thus ignores a long, fascinating era which culminated in America's greatest growth rate per capita while the nation subscribed to no dominant philosophy of work but, rather, entertained a series of post-

Enlightenment exhortations toward spiritual perfection and transcendental self-realization. Oversimplifications aside, Snowman's list of traditional values is neither original nor debatable.

Snowman's successes reflect his skills as a polite essayist and result from some fresh and surprising combinations of personal insights, observations, and allusions. Although there are sound historical reasons for arguing against his assertion that the peculiarly *modern* American values are "technique infatuation," "transitoriness," "participatoriness," and "self-indulgence," there is much to be gained by following the argument through discussions of the decline of athletic amateurism, the similarities between Monty Python and Laugh-In, and the revealing idiosyncracies of idiom that allow a British politician to "stand" for office while his American counterpart must "run." (Snowman would enjoy Henry W. Shaw's hundred-year-old observation that the American also "works, eats, and haw-haws on a canter," elaborating through humor what the Englishman "biles down" to wit.)

A minor misfortune may be traced to whatever impulse led the author to encrust his often lively essay with overblown protestations of interdisciplinary methods and intercultural devices. This pretentiousness is crowned in a paragraph of self-comparison with Marx, Bagehot, Turner, and Toffler (p. 258). A major and much more puzzling misfortune is in the failure of a university press to insist on an even and functional practice of documentation or on a bibliography which would not only place this work in its proper context but might also raise the highly germane question of why those tons of biased, personal views of one culture, as seen from the other, have so far outweighed such thoughtful, dispassionate comparative studies as the present one.

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DOUGLAS T. MILLER and MARION NOWAK. *The Fifties: The Way We Really Were*. New York: Doubleday and Company. 1977. Pp. viii, 444. \$10.95.

With more care, restraint, and reflection, this could have been a superb book. Impressively researched, imaginatively put together, and passionately written, *The Fifties* is a thorough social and cultural history of a decade that thus far has inspired more in the way of nostalgia than insightful commentary. Coauthors Douglas T. Miller, a professional historian known hitherto for his work in the Jacksonian period, and Marion Nowak, a freelance journalist, treat American politics and foreign relations in the 1950s only offhandedly, but

they do manage to cover practically everything else: McCarthyism, the Cold War consensus, and intellectual acquiescence; patterns of economic, religious, sexual, and racial behavior; preoccupations with the family, adolescence, and the automobile; educational controversy; music, television, motion pictures, and fiction. Except that "the anxieties of the fifties could only continue to grow into the righteous affirmations of the sixties" (p. 389), they see almost nothing good in the years 1950-60. "It was," they believe, "a modulated age; everyone seemed smothered in a blanket of inertia, apathy, and conformity" (p. 131). "These were the years of security checks, of conformity trampling over personality, of individuality allowed only within a narrow norm" (p. 190).

Such sweeping, simplistic characterizations indicate the major problem with their book. Miller and Nowak persistently lecture more than they describe, strive to persuade more than to explain. Some of their claims are far fetched, such as that in the fifties big breasts were symbols of motherhood, or that government repression of Marxist radicalism contributed to juvenile delinquency. And one supposes their occasional errors of fact result from carelessness rather than ignorance. The atomic artillery shell was not "an expensive dead end" (p. 45) but already an operational weapon by the mid-fifties. *The Robe* (1953) was the first movie in CinemaScope, not *The Ten Commandments* three years later. J. D. Salinger's *The Catcher in the Rye* was first published in 1951, not 1956.

Yet for all its faults, big and little, *The Fifties* has much to offer. Miller's and Nowak's chapter on the systematic efforts by government and the popular press to de-horrify public thinking about nuclear explosives is excellent, as is their treatment of popular music, even if one is not convinced by their claim that rock and roll was a form of generalized social protest. For those who lived through the 1950s, their volume's presumptuous subtitle might prompt the question, "Was it really that awful?" Probably not, but as the mass of factual material presented by Miller and Nowak makes abundantly clear, it was bad enough.

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CANADA

BRUCE G. TRIGGER. *The Children of Aataentsic: A History of the Huron People to 1660*. In two volumes. Montreal: McGill-Queen's University Press. 1976. Pp. xxiii, 453; x, 455-913. \$45.00 the set.

Bruce Trigger has written a history of the Huron people of central Ontario covering the period

roughly 1530 to 1660, based on contemporaneous accounts by European observers. The purpose of the book is to present the history of the Hurons from their own perspective rather than from that of Euro-Canadians who, then as now, tended to be very naive in their conception of Indian economics, politics and social complexities. As Trigger rightly says, "Indians have been viewed, both collectively and individually, less as actors on the stage of Canadian history than as part of the natural setting against which this action has taken place." One of the book's contributions is a convincing demonstration of the fallacy of this view.

Following the introductory statement of aims and methods are chapters dealing with the geography of the Huron country, ethnographic descriptions of the Hurons and their neighbors, and the archeological background of the Hurons. The author presents a careful analysis of early contacts between Europeans and Indians on the St. Lawrence River and shows how these contacts affected Huron society even prior to the arrival of the French in Huron country. Trigger also details the Huron-French contacts from 1609 to about 1630, during which time military and economic links between the two peoples were forged. Of particular interest is the discussion of the internal politics of Huron society, and how these helped to shape Huron policy toward the French.

Other chapters cover the period from about 1630 until just prior to the Hurons' dispersal by the Iroquois in 1649. Several things make these chapters noteworthy. First is the discussion of the Hurons' attitudes toward Christianity, the Jesuit missionaries, and the French in general, all of which shaped their policies and behavior. Second, Trigger presents what I found to be a fascinating and convincing analysis of the early stages of the Iroquois' war against the Hurons, including a discussion of the Iroquois' motives, their military strategies, and the complexities of the behind-the-scenes diplomacy among the Hurons, French, Algonquins and Mohawks during the 1630s and early 1640s. Finally, Trigger presents an analysis of some major changes in Huron society between 1640 and 1647, and shows how these contributed to the Huron defeat in 1649 and how the Jesuit missionaries unwittingly played a key role in bringing them about.

The author describes the forging of a common anti-Huron policy among the Seneca, Mohawk, and Onondaga, and the subsequent swift execution of this policy, culminating in the destruction of the Hurons as a social and political entity in 1649. He also briefly recounts the fate of various segments of the Huron people following their defeat by the Iroquois.

Anthropologists who seek in this work new inter-

pretations of Huron ethnography and ethnohistory may be disappointed. The book has, I think, two major contributions. The first is its demonstration of the degree to which the institutions and dynamics of Huron society shaped the events of the sixteenth and seventeenth centuries in Canada. Second, Trigger's history represents a major step toward a sympathetic understanding of the Hurons. The reader can hardly fail to come away from this book with a more vivid picture of those early Canadians as believable people with virtues and faults, and with a realization that their behavior is no more and no less comprehensible than that of their European contemporaries who unwittingly and unthinkingly fashioned posterity's image of the Huron. As an attempt to focus that image, Trigger's latest work must qualify both as good anthropology and good history.

PETER G. RAMSDEN
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LATIN AMERICA

DANIEL COSÍO VILLEGAS, general editor. *Historia general de México*. In four volumes. (Centro de Estudios Históricos.) Mexico City: El Colegio de México. 1976. Pp. viii, 288; viii, 446; viii, 331; viii, 505.

This four-volume general history of Mexico produced by El Colegio de México suggests two important facets of the life of the late Daniel Cosío Villegas, distinguished Mexican historian. Cosío was, in the words of Enrique Krause, a "cultural entrepreneur." The present set of books is one more example of his tendency to initiate, promote, and coordinate worthwhile endeavors of cultural or intellectual importance. Further, the volumes also represent his strong commitment to define his nation's past and to inform his fellow Mexicans so that they could better understand their present and work toward a better future.

The Center for Historical Studies at El Colegio de México has had a distinguished record of intellectual achievement through the training of historians, the carrying out of research, the publication of monographs, and the initiation and publication for more than a quarter of a century of a respected journal, *Historia Mexicana*. However, it was Cosío's and El Colegio's hope to reach a wider audience than had been achieved through scholarly publications. The initial step was the publication of a 164-page *Historia mínima de México* as a kind of introduction to Mexican history. Based on that text, a "television" of Mexico was prepared and telecast on three channels, reaching an even wider audience.

It was with the same goal of a wider dissemination of knowledge of Mexican history that the current volumes were prepared—a general history for the general reader. Each segment or chapter was written by an individual with specialist knowledge of the theme. Each undertook the assignment with the understanding that the text should be easy to read and to understand. Inevitably, as in any multi-authored work, there is a fairly wide variation in the quality and character of the presentations, ranging from those simply detailing factual information to others of a broadly interpretive nature. The coordinator recognized that there was, at times, a tendency for the essays to overlap when authors discussed the roots or consequences of their assigned periods. It was decided, however, to leave the texts as they had been prepared, both in the interest of time and in an effort to avoid adversely affecting the originality of the essays.

The initial volume consists of four sections on geography, prehistory, and preconquest Mexican society. Against the background of Bernardo García Martínez's description of Mexico's geographical reality, José Luis Lorenzo describes the gradual extension of our knowledge of the origins of man in the area. Ignacio Bernal details the formation and development of Mesoamerican civilizations, and Pedro Carrasco examines Mexican society before the conquest. That conquest is imaginatively told in a dialogue between Spanish and native sources by Alejandra Moreno Toscano in the initial essay of the second volume. That is followed by a discussion of the establishment of the economic bases of New Spain in the seventeenth century written by Andrés Lira and Luis Muro. The Bourbon reforms of the latter half of the eighteenth century receive attention from Enrique Florescano and Isabel Gil Sánchez in an essay that concludes with the growing evidence of social and political instability early in the nineteenth century leading directly to the independence movement described by Luis Villoro. His essay is based in part on his earlier publication in which he examined the ideological process of the independence movement.

Four essays in volumes two through four are devoted to cultural and artistic manifestations. Jorge Alberto Manrique contributes two of the sections. His narration of the cultural evolution from the baroque to the Enlightenment, including architecture, painting, sculpture, and philosophy is a *tour de force*. There follows a review of religion, education, and urban life in the period of the Enlightenment. The same author treats the development of the plastic arts in the first seven decades of the twentieth century in the fourth and final volume. In the third volume, José Luis Martínez, seeking to delineate and synthesize nineteenth-

century culture, concludes that during the first independent century Mexico was searching for means of expression. Lastly, in characteristically amorphous form and critically ironic expression, Carlos Monsiváis offers his notes on selected aspects of Mexican culture—literature, philosophy, ideology, theater, and film—in the twentieth century.

The third and fourth volumes treat the political, economic, and social history of independent Mexico. In a particularly perceptive essay, Josefina Zoraida Vázquez examines the obstacles faced by newly independent Mexico: international (recognition and the ambitions of the great powers), political (the establishment of the state), a bankrupt economy, and the society (in terms both of its heterogeneity and its structure). Lilia Díaz provides a straightforward account of the post-1848 period including the Reform movement, the French intervention, and the Maximilian episode. With his consummate literary skill, Luis González describes the Restored Republic and the period of the Porfiriato. Here is a synthesis of the multi-volume *Historia moderna de México* which Cosío's seminar at El Colegio produced. González concludes with the decline of the Díaz regime.

The background for the Mexican Revolution is treated in solid essays by two scholars whose monographic work is well known to specialists on twentieth-century Mexico. The armed phase of the Revolution is the focus of Berta Ulloa's interest, while Lorenzo Meyer in two essays synthesizes the establishment of institutions and external relations in the two decades after 1940 and the evolution of industrialization and urbanization since the Second World War.

The volumes under review accomplish the purpose for which they were written. They should be very helpful to Mexican students at the high school and university level as well as to adult readers who give them the time and attention they deserve. I hope that they will also come to the attention of Spanish-reading students in the United States interested in Mexican history.

STANLEY R. ROSS
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JAN BAZANT. *A Concise History of Mexico from Hidalgo to Cárdenas, 1805-1940*. New York: Cambridge University Press. 1977. Pp. x, 222. Cloth \$14.95, paper \$4.95.

In this latest attempt at a synthesis of modern Mexican history, Jan Bazant sees the struggle among peasants, the middle sectors, and the upper class over land as the dominant theme of Mexican

history from 1805 to 1940. His thesis is hardly new, for forty years ago and more Frank Tannenbaum perceived agrarian struggle as the central theme in Mexico's history since independence. While Bazant's thesis lacks originality, his is a carefully researched and clearly written narrative which takes into account much of the research on nineteenth- and twentieth-century Mexico published in the last ten years.

Inevitably a synthesis must include considerable material familiar to the specialist, but it is remarkable how much fresh data Bazant's work contains. This is especially true of the first three chapters, which examine the independence movement, the chaotic Santa Anna era, and the liberal revolution of 1855-76. In the first chapter, Bazant argues persuasively that the royal decree of December 12, 1804 calling for the compulsory redemption of mortgages belonging to chantries and for pious works in Spanish America estranged Mexican landowners and led them, along with members of the lower clergy, to promote a movement which eventually led to Mexican independence. Bazant eschews the popular but anachronistic view of Hidalgo and Morelos as "agrarian reformers" for lack of evidence, and demonstrates instead that Hidalgo suffered directly from the 1804 Church consolidation. In the succeeding two chapters Bazant clearly shows that the assault on Church property began as early as 1821 and was the work of middle-class liberals striving for land and status. In the end these liberals formed an economic alliance with conservative upper-class landowners and turned their backs on the lower classes years before Porfirio Díaz came to power in 1876. Ironically enough, it was Maximilian and not Juárez and the liberals who sought to end debt peonage and to protect Indian *ejidos*.

The remaining half of the book, made up of three chapters on the Porfiriato, the Revolution of 1910-20 (which Bazant views as a civil war rather than a revolution), and the postrevolutionary reform era to 1940, is less incisive than the first half. Here, for example, it is difficult to agree that Porfirio Díaz was "a good and effective patriot" (p. 114) because he divided valuable petroleum concessions between American and British investors rather than permit investors from only one country to control Mexico's oil. There is also a certain naiveté in Bazant's judgment that there was never any reason to doubt Madero's honesty or that Carranza's revolt against Huerta "could not have been motivated by thirst for power or money" (p. 154). But these are minor flaws in an authoritative and readable account of Mexican history from Hidalgo to Cárdenas.

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CHARLES A. WEEKS. *El mito de Juárez en México*. Translated by EUGENIO SANCHO RIBA. Mexico City: Editorial Jus. 1977. Pp. 231.

This book is a revised and expanded version of Charles Weeks' dissertation, "The Juárez Myth in Mexico." Beginning with an analysis of the psychological and sociological functions of myths in history, Weeks proceeds to narrate the history of the heroic symbol of Benito Juárez from Juárez' political triumph in 1867 to the beginnings of the Luis Echeverría administration in 1972. In his survey, Weeks notes that official manipulation of the Juárez myth first began in 1887 during the Porfiriato and continued up to the Centennial of 1906 when historians Justo Sierra and Rafael de Zayas Enríquez venerated him in oratory and biography, equating Juárez and Porfirio Díaz with the progressive, liberal forces of the nation.

Not surprisingly, a few years later both Francisco Madero and the Constitutionalists manipulated the symbol to justify their own anti-Díaz struggles and to claim for their political movements the authority and continuity of the Juarista tradition. Although the revolutionary muralists of the early 1920s perpetuated the myth, the government of Lázaro Cárdenas (1934-40) made only limited use of the symbol, rejecting Juárez' nineteenth-century liberalism for the native socialism of the 1930s. Only during World War II did the Mexicans turn once again to the symbol in order to restore their lost sense of nationhood. Today the reality of the myth of Juárez is expressed through those values which equate the Juarista tradition with indigenism, constitutionalism, nationalism, secularism, and civilism.

Weeks' central thesis is that the Juárez myth, like most myths, reflects a subjective view of reality, and is an embodiment of a kind of Mexican self-consciousness and awareness having psychological, social, and political uses. As Weeks demonstrates, many of the mythical expressions were polemical, resulting in the creation of a history of pro- and anti-Juarista intellectual traditions. Although these are not original observations, they are worth noting and well grounded in the traditional authority of such thinkers as C. G. Jung, Emile Durkheim, Bronislaw Malinowski, and Murray Edelman.

The author has succeeded well in adapting European theory to American reality; his book provides a concise and interesting account of this dimension of *Juarismo*. Based on research in archives and libraries in Mexico and the United States, Weeks' account of the political ideologies of a variety of Juaristas relies primarily on newspapers, journals, and pamphlets published between 1872 and 1972. The publishers should be complimented

for producing an attractive work—well illustrated with photographs and caricatures—and for keeping intact the author's most informative and inclusive bibliography.

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K. E. INGRAM. *Sources of Jamaican History, 1655-1838: A Bibliographical Survey with Particular Reference to Manuscript Sources*. In two volumes. Zug, Switzerland: Inter Documentation Company. 1976. Pp. xvii, 1,310. 176 FR.

This survey of the sources on Jamaican history is a particularly useful bibliographical tool. Kenneth Ingram, who heads the library at the University of the West Indies in Jamaica, has concentrated on the repositories in Britain and Jamaica and to a lesser extent on those located in North America and Europe. He has avoided the temptation of covering only the most accessible holdings or those which are best known; instead, he also draws attention to manuscripts in county record offices, in universities and other institutions, and in private hands. The result is a masterly piece of work.

In a substantial introductory section, Ingram reviews the existing bibliographical literature and discusses the types of material to be found in the various archives and libraries. He notes that West Indian scholarship has relied in the past on general guides designed for American, Latin American, and British colonial specialists; his book not only recognizes the shift toward Caribbean studies but will also make research in the field that much easier.

Ingram has arranged the material by subject headings: these include historical and descriptive accounts, politics and government, church history, plantation papers, slavery and the slave trade, and business papers. Wherever possible, he has personally examined the records; his descriptions are thorough, detailed, and often interesting. In addition, he has reported on several collections which have been largely neglected; among these are extensive plantation and business records in England. The additional material he has uncovered on the slave period should be of great help to scholars working in this area.

Ingram is not satisfied with surveying manuscripts. He adds supplementary lists of lesser-known documents on Jamaica in the Public Record Office, guides to other bibliographies, sources for Jamaican newspapers and periodicals, and indexes to relevant parliamentary proceedings and sessional papers. Since Ingram's book is a welcome addition to the literature on Caribbean bibli-

ography, historians of the region will be grateful to him for it.

GAD J. HEUMAN
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OSCAR LEWIS *et al.*, editors. *Four Men, Living the Revolution: An Oral History of Contemporary Cuba*. Urbana: University of Illinois Press. 1977. Pp. lxxv, 538. \$15.00.

This work and its companion volumes (*Four Women; Neighbors*) are the last works of the anthropologist Oscar Lewis, who pioneered the ethnographic study of the urban poor. *Four Men* contains a distillation of taped interviews recorded by Lewis, his wife Ruth, and their Cuban staff in Havana during 1969 and 1970. The men whose lives are related in this volume come from the poorest stratum of prerevolutionary society and are now residents of a community known for its lack of integration into the revolutionary process.

Everything that touches the Cuban revolution tends to become political. The reader familiar with Lewis' theories concerning the pathology of the poor will view this book as a confrontation between the "Culture of Poverty" and the "New Socialist Man." Indeed, this was the idea which motivated Lewis to investigate the impact of the revolution on Havana's former slum-dwellers.

Even before Lewis brought his ethnographic realism to the charged atmosphere of the Cuban revolution, his culture of poverty concept had left a trail of academic discord in its wake. While Lewis clearly sympathized with the poor whom he studied, his idea that for the long-term poor poverty had become a way of life was, in other hands, a reason for accepting their exclusion from the mainstream economy. Since their behavior was, from the perspective of grand culture, "pathological," the proper societal response was the work of courts, social workers, and psychiatrists. Though Lewis emphasized that the cultural patterns of the poor were determined by the structure of the society as a whole, his books seemed to reveal only their incorrigibility. By focusing on the slum family and recollections of personal relations, they contain a litany of sins against bourgeois decency (casual unions, illegitimacy, impulsiveness, fatalism).

The microscopic nature of Lewis' ethnography in *Four Men* subdues revolutionary forces and accentuates "daily life" where, on the basis of the included evidence, much of the "pathology" of the culture of poverty survives. This intimate approach obscures the adaptive nature of this apparently disorganized world and draws attention away from the forces which make such behavior necessary.

If the culture of poverty arises as a response to prevailing social roles by those systematically closed off from fulfilling them, then the place to seek out changes in their situation is not at the core of these responses but rather at those new junctions where a revolutionary state seeks to make possible (and even demand) such fulfillment. That is, one must leave the home (where interpersonal habits are most insulated from new social expectations) and enter instead the block committee, the work place, and the local party headquarters. Here the once marginal may experience new dominant models and apprehend that the state now operates to ensure rather than frustrate their fulfillment.

Ironically, the subcultural traits of the slum-dwellers, which under the former regime eluded the corrective efforts of jailers and poverty-workers alike, are, at this stage of the revolutionary process, among the "antirevolutionary" habits which socialist morality is trying to combat. The bourgeois traits (stable nuclear family, steady employment, punctuality) which the pre-revolutionary society had required of, and yet made impossible for, the poor are now components of the new socialist citizen. The evidence in this volume, despite its subinstitutional focus, indicates that Cuban socialism may be succeeding precisely where its predecessors failed.

JULES R. BENJAMIN
University of Rochester

O. NIGEL BOLLAND. *The Formation of a Colonial Society: Belize, from Conquest to Crown Colony*. (Johns Hopkins Studies in Atlantic History and Culture.) Baltimore: Johns Hopkins University Press. 1977. Pp. xiv, 240. \$15.00.

Six of the twelve chapters of this valuable monograph deal with slavery, as do the six appendixes. Such emphasis is appropriate since slavery in Belize (British Honduras), based on timber-cutting rather than the plantation, differed significantly from that of the insular Caribbean. This resulted in such obvious differences in social patterns that Belize slavery gained the reputation in popular and scholarly literature of being less oppressive than plantation slavery. O. Nigel Bolland is at pains to disprove that idea. He concedes that the daily life of the woodcutter slave of Belize was freer in certain respects than that of the plantation slave—relative freedom of movement, the custom of slaves bearing arms, and close acquaintance with the master (there were few absentee landlords). All of this was conditioned by the real possibility of escape to Mexico, to Guatemala, or just to the forest. The author is concerned, however, that this objective situation should not be

imputed to the relative humanity of masters in Belize. The role of "the huntsman" alone (the slave who located groves for cutting) indicates the unique character of slavery in Belize, but Bolland argues persuasively that the coercive base of logging slavery was essentially identical to that of slavery elsewhere in the Caribbean.

Bolland emphasizes the years 1765-1871, the most important period for the study of slavery, emancipation, and the post-emancipation generation. In his zeal to illustrate the racism which dominated this entire century in Belize he seems almost to blame the slave-owners for *not* developing the plantation, which in Jamaica laid the base for an independent peasantry after emancipation (see, for example, pp. 61 and 124), though obviously this was not his intention. Benét's quatrain beginning "Bury the unjust thing," would convey the view Bolland takes of Belize's social traditions derived from the timber industry.

Research in archives as well as in specialized and general literature makes this work a major contribution to our knowledge about colonialism, the Caribbean, and Belize. Particularly valuable is the author's presentation and analysis of documentation in Belize and in British Colonial Office records. This work is a direct challenge to that of Narda Dobson, *A History of Belize* (1973). Bolland and C. H. Grant (*The Making of Modern Belize*) represent a new generation of skillful and determined writers who are changing the historiography of the Caribbean by using sophisticated techniques to write "people's history."

WAYNE M. CLEGERN
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WAYNE D. BRAY. *The Common Law Zone in Panama: A Case Study in Reception*. Introduction by GUSTAVO A. MELLANDER. Prologue by ALFONSO L. GARCÍA MARTÍNEZ. San Juan, P.R.: Inter American University Press. 1977. Pp. xxiii, 150. \$20.00.

This history of the changeover from civil law to common law, or the "reception" of the latter, in the Panama Canal Zone, belongs primarily in international law libraries. The author, an attorney, begins by exploring the origins of the civil law system which functioned in the Colombian province of Panama and points out how it differed from common law. He then refers to specific cases to illustrate how reception evolved after the United States took over the Zone. The study shows that while the canal was being constructed (1904-14) the U.S. established a legislative rather than a constitutional court system for the Zone, and that implantation of civil law proceeded along with other aspects of cultural domination, including English becoming the enclave's official language.

Subsequently, legislation, the presence of more U.S. citizens, and altered land tenure patterns reinforced common law principles in the Zone. Eventually, *stare decisis* strengthened reception, even though law by precedent was not entirely new in the region where under Spanish custom a series of concurring decisions had constituted *doctrina legal*. Statutory civil law in the Zone disappeared in 1935, but its influence remains. The author assumes that the Zone will revert to Panama, and that civil law will return there, a situation unique in the history of jurisprudence.

The book's subtitle promises "some observations on the Panama Canal controversy," but the writer presents no new or particularly sagacious insights. For the most part his diplomatic history is stale, although he cogently analyzes the Hay-Bunau-Varilla Treaty, categorizing it as *unlegal*, rather than *illegal*. Also, he masterfully interprets the meaning of sovereignty as applied to the Canal and Zone and concludes that sovereignty resides in Panama, but that the U.S. has exercised some attributes of it, and that the current jurisdictional impasse can only be eliminated by U.S. withdrawal from the contested area.

The superfluous introduction and prologue overstate the book's contemporary value, and indicate, erroneously, that it reads like a novel. The author senses that he has provided considerable overdocumented and extraneous detail, but does not appear to realize that clichés and folksy asides diminish his thoroughly researched treatise. Although he organizes facts and chronology well, he rarely differentiates between the historically significant and the tangential, and at times fails to analyze causal relationships. He tells a great deal about the adaption of civil law to Panama and Latin America, and admits that, in so doing, he leaves many questions unanswered. This volume would be more complete had the writer used his immense knowledge of Panama's social and legal institutions to evaluate critically the effects of reception on U.S.-Panamanian relations, and on social justice and the human condition in the Canal Zone.

SHELDON B. LISS
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MIGUEL IZARD *et al.* *Política y economía en Venezuela, 1810-1976*. Caracas: Fundación John Boulton. 1976. Pp. xvi, 292.

At the 1975 annual convention of the Southern Historical Association, a Venezuelan scholar commented that research on the chaotic nineteenth century in Venezuela was a waste of time. This work, however, contains six articles (seven if one agrees with José Antonio Mayobre that the twentieth

eth century began only with the death of Juan Vicente Gómez in 1935) on the nineteenth century that could hardly be considered scholarly waste. Four of the essays, taken from recent doctoral dissertations by U.S. historians, have been translated from the original English; three are by Venezuelans—including one by Mayobre on the period since 1935—and one is a synthesis of the Spanish historian Miguel Izard's work on Venezuelan independence. A theme common to the eight essays gives the book an unusual unity; each historian treats the impact of international economic fluctuations on the Venezuelan economy and politics and evaluates the degree of skill with which Venezuelan presidents have coped with that impact.

Within the common theme, each author employs a slightly different focus. Manuel Pérez Vila, Director of Historical Research at the Fundación John Boulton, stresses the impact on Venezuela of external events such as Andrew Jackson's attack on the Bank of the United States. Similarly, Nikita Harwich Vallenilla traces some of the repercussions of German financial expansionism in Venezuela in the 1888–1908 period. Miguel Izard on the 1810–30 period and Mary B. Floyd on the 1870–88 period explore the relationships between different social and economic groups and the power elite in Venezuela. Robert Matthews finds that deteriorating rural economic conditions contributed to the fall of the Monagas brothers (1847–58), but Benjamin Frankel places the blame on the corruption and inefficiency of José Tadeo and José Gregorio Monagas; moreover, Frankel argues that the subsequent Federal War, a meaningless civil eruption prompted by irresponsible *caudillos*, also had its roots in the Monagas maladministration. Finally, William Sullivan and J. A. Mayobre examine the changes brought to Venezuelan politics and economic structure by the development of the petroleum industry and ever increasing integration into the world economy.

The essays simultaneously provide greater insight into the nineteenth-century history of Venezuela and suggest new directions for research. For example, Frankel implies a connection between the corruption of the Monagas family and the economic distress of the country, but Floyd describes a generally prosperous nation and a skillful administration under Antonio Guzmán Blanco, despite Guzmán's use of his power for personal enrichment. Can any relationship be established between corruption, political effectiveness, and international economic cycles? If so, does the relationship remain constant in Venezuela during the nineteenth century? It is probable that even a tentative answer to such questions would have to apply sophisticated quantitative techniques to supplement the traditional historical methodolo-

gies used by most of the authors whose work is included in this volume.

In sum, this book is a valuable compilation of recent research on Venezuelan history. Additionally, it might provide a model for U.S. scholars who want to make the results of their research easily accessible to historians in the host country. In many of these essays the reader would have to turn to the original dissertation or book for full bibliographic citations, but the summary of research and interpretations in Spanish does provide an important avenue of communication between Venezuelan and foreign scholars with similar interests.

JUDITH EWELL

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R. J. BROMLEY. *Development and Planning in Ecuador*. London: Latin American Publications Fund; distributed by Grant and Cutler, London. 1977. Pp. ix, 116. £ 2.80.

The thesis of this short monograph is that nearly twenty years of Ecuadorian development planning has been a failure, used by successive regimes primarily to acquire loans and gain acceptance. The planning apparatus largely echoes the hypocrisy and antidemocratic behavior of Ecuador's small but often fragmented ruling elites. Plans, therefore, do not simply fail; they are ignored and do not reach the forums where their elaborate schemes might be formulated into actual policy and implemented.

R. J. Bromley's study succeeds in three general areas. Including an extensive bibliography, it surveys briefly the socioeconomic situation of the country and gives the survey a sound historical context. Second, Bromley argues well for Ecuador as a case study of dependency in economic and ideological terms, a dependency that perpetuates and deepens social and economic inequality. Third, the volume presents a sound, if abbreviated, description and analysis of development plans and planning. This is framed in terms of Ecuador's social and racial composition, distribution of resources and power, and political problems of authority and control since 1948. Attention is repeatedly drawn to the paradox that Ecuador profited substantially from petroleum revenues during 1972–75—that is, until Texaco-Gulf reduced output from the oil fields controlled by the giant firm—but could not diminish either dependency or the often painful inequalities among its more than seven million people.

The author's intellectual framework is that of the so-called dependency school. This does not severely limit perception, since Bromley considers

options available to Ecuadorians and the reasons for their relative lack of success. While he begins with a superficial definition of development ("change through time," p. iii), Bromley indicates the larger dimensions he intends to be covered by the concept—wider and more popular social and political participation, pluralism, positive social activity directed toward redressing inequality, national integration, improved sovereignty, and the like.

Thus the volume is likely to benefit the general reader, if only for its synthesis of Ecuadorian conditions and the clear presentation of additional material in nearly a score of charts and maps. For more advanced students, however, the study's shortcomings are its brevity and the attendant lack of sustained exposition concerning Ecuador's political economy and its practically unrelated planning function.

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JUAN MARTÍNEZ-ALIER. *Haciendas, Plantations and Collective Farms: Agrarian Class Societies—Cuba and Peru*. (Library of Peasant Studies, number 2.) London: Frank Cass. 1977. Pp. viii, 185. \$19.50.

During the past two decades a great deal of new research has been published on the rural history of Latin America so that a subject long ignored now has a respectable body of literature. This new research, carried out by students from three continents, has been progressively examined, revised, and refined in specialized congresses in Rome, Cambridge, Mexico, and Paris, and it is fair to say that a certain understanding of rural structure and process in Latin America is now emerging. As work in this subfield matures, two recent developments are worth noting: new evidence such as estate accounting records and private correspondence is coming to light; and researchers are finally shedding their self-imposed Marxist-liberal disdain for the landowning class which made it difficult to put themselves, even for a moment to see how it felt, in the boots of the black-hearted *hacendado*.

The present book, dealing with Peru and Cuba, is an excellent example of the new research. A Catalán by birth, trained in Spain and at Stanford and Oxford, and experienced through field research in Andalusia, Cuba, and Peru, Juan Martínez-Alier places himself squarely at the center of the culture he seeks to understand, asks a skeptical economist's questions, and then has the good sense (and the sources) to permit the historical actors themselves to answer. All this is reported

in language which is at once modest, engaging, and slightly tangled.

The two chapters on highland Peru are based on accounts and letter files of private estates confiscated by the recent (1969) agrarian reform and now catalogued in a special archive in Lima. It is this kind of material which permits Martínez-Alier to explain the real choices faced by both landlord and peasant and to demonstrate the limits to action imposed by public opinion and social reality. Even in this presumably oligarchic and boss-ridden despotism (the estate records run mainly from the 1920s to the 1960s) the landowners were frustratingly unable to expel their supposedly powerless tenants, in large part because the tenants (often thought to have been bound to the estate by peonage) knew that they were far better off as "serfs" than as modern wage-workers, and so they clung tenaciously to their oppressive condition. When eviction is punishment it is no longer possible to accept the old notion that debt was needed to hold labor. These questions of labor use, exploitation, and the meaning of surplus are considered in chapters two and three against the evidence and also in a long introductory discussion of theory. It is the mix of hypothesis and research and careful qualification which makes the book especially interesting.

The section on Cuba is also based on field research and, most importantly, on the Cuban files of the revolutionary agrarian reform itself. The chapter on the cane planters (1934–60) points out the importance of this intermediate layer of Cuban society (between the powerful, often foreign, mill owners and the workers) in shaping Fidel Castro's early ideas about agrarian reform; the following chapter convincingly argues that the landless and often unemployed workers themselves were instrumental in pushing the reform in a more radical direction; and the final chapter discusses in a respectful, but always independent and skeptical way, the nature of Cuba's present economic and political problems.

These—in 450 words or less—are the main features of Martínez-Alier's work. It is a slender and deliberately modest book, but because its unusual sources, thoughtful questions, wit, and common sense are so engagingly integrated, it is a work of exceptional value, challenging for Latin Americanists and useful for others.

ARNOLD J. BAUER
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 Davis*

FREDRICK B. PIKE. *The United States and the Andean Republics: Peru, Bolivia, and Ecuador*. (American Foreign Policy Library.) Cambridge, Mass.: Harvard University Press. 1977. Pp. xv, 493. \$25.00.

Fredrick B. Pike's interesting work claims much more than it proves, but it could not be otherwise in a study of such breadth. The author has analyzed the political and economic developments of Peru, Ecuador, and Bolivia that have taken place under the patronage of the U.S. whose leadership has never fully comprehended what was taking place in these countries. Pike thus outlines the growth of two significantly different value systems. He points out that in the Andes national and international developments have always depended heavily on patron-client patterns, in contrast to the U.S. where the tendency has been to glorify individual independence. In a final challenge, Pike asks: "Is Andean America [with its collectivist society] more ready than the U.S. to enter a post-modern era in which people . . . may have to learn increasingly to settle for nonmaterial satisfaction?"

This is essentially a work of synthesis, based primarily on secondary sources. One of its major merits is the running commentary on historiographical questions which frequently accompanies the narration of national and international events. The title is slightly misleading; comparisons of the three nations with the U.S. are interspersed throughout the narrative rather than integrated into it.

The author contends that "corporativism" (neither capitalistic nor communistic in orientation) is the system most congenial to Andean cultural traditions and is therefore the best vehicle for moving that region into the postmodern era. Pike analyzes the pre-Columbian, colonial, and early national historical forces which shaped the growth of the three societies, creating at the top of the pyramid an elitist, patron-like leadership which always directed the dependence-deference masses at the bottom. No segment of Andean society found anything particularly attractive in competitive individualism, liberal democracy, or capitalism. U.S. political and economic intervention, public or private, direct or indirect, has had no significant influence on basic national patterns. Nevertheless a pretense of anti-Communism or liberal democracy has occasionally been feigned for the purpose of gaining economic assistance or support from Washington against a neighbor. Although the power elites of Andean corporativism have long sought to escape, or said they did, from economic dependence on the U.S., the dependence of industrializing nations on postindustrial nations may well continue; in that case Peru, Ecuador, and Bolivia can only strive to render the North American giant a more generous *patrón*.

A certain amount of confusion has resulted from the editorial handling of the manuscript at one stage or another—for example, lack of punctuation

and numerous typesetting and/or proofreading errors. A more substantial criticism is that Pike's depiction of Víctor Raúl Haya de la Torre is unnecessarily pejorative; the Aprista leader is said to "wax didactic" while asserting "domination" over the "mob and mediocre men," and to be unwilling "to confess a shortcoming or admit a mistake, for this would have destroyed the whole mystique he was trying to create as the leader set apart from the rest of mortals and endowed with charisma" (pp. 227, 228).

Notwithstanding these criticisms, Pike's work is a major and valuable contribution which will help bring about an understanding of the forces and ideas shaping the destiny of this section of South America. The scholarship is basically sound and the book is heavily documented, but the breadth of premise defies any tidy statement of argumentation. This study will help both the general reader and the scholar to appreciate the historical traditions of Peru, Ecuador, and Bolivia. It is a work that deserves to be read, and which will be important for some time to come.

JAMES C. CAREY

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EDWARD BOORSTEIN. *Allende's Chile: An Inside View*. New York: International Publishers. 1977. Pp. x, 277. Cloth \$12.50, paper \$4.25.

DAVID CUSACK. *Revolution and Reaction: The Internal Dynamics of Conflict and Confrontation in Chile*. (Monograph Series in World Affairs. Volume 14, book 3.) Denver, Colo.: University of Denver Graduate School of International Studies. 1977. Pp. xi, 146. \$3.50.

After the overthrow of Salvador Allende's socialist government by the Chilean military in 1973 a host of books and articles appeared dealing with the Allende experiment and its early demise. Unfortunately, most of these works have been emotional and partisan. Now, however, more objective studies are beginning to appear. David Cusack's monograph on the Allende years is a thoughtful assessment of the political, economic, and social dislocations of this period. He argues that even before Allende won the presidency in 1970 the seeds for his overthrow were sown as numerous political and economic factions polarized. Strikes and demonstrations for higher wages became commonplace and in 1969 even some army units protested for economic benefits. This atmosphere of confrontation politics continued throughout the Allende presidency, bringing more chaos to the nation as middle-class, professional elements took to the streets to apply pressure on the socialist government. Meanwhile, the left-wing workers' or-

ganizations continued to rely on the same tactic, thereby adding to the general tumult which by 1973 made governance of the nation next to impossible. In this tense situation the military became crucial. When the officers concluded that their institution was threatened they toppled Allende. Some writers have charged that Allende did not act decisively enough in 1973 and that had he armed his civilian supporters he could have headed off the assault on his government. Cusack dismisses this contention by pointing out that at no time were the civilians, with only about 5,000 armed men, capable of challenging the military might of 125,000 well-equipped troops.

Cusack laments the military action because it brought a return to the old pattern of in-group, out-group politics. This, coupled with repressive tactics, has led Chileans to lose trust in their political system which had long been a source of pride for them.

Edward Boorstein presents a less objective study of Allende's administration. He finds the United States to blame for Allende's fall rather than internal politics. He states that the United States was the "principal force" behind Allende's destruction. The multinational corporations, the State Department, and the CIA all contributed to the military intervention that ended the socialist experiment. While this view is in keeping with the general Marxist position, Boorstein does not criticize Allende for failure to move more quickly toward a socialist state, as do some socialists. He argues rightly that with thirty-six percent of the vote in 1970 Allende did not possess the power base necessary to bring socialism immediately to Chile. Therefore Allende's compromises with the opposition Christian Democrats proved to be a wise course of action. Likewise, Allende's attempt to curry favor with the military, often criticized by

socialists, was a valid strategy because the military was always a threat, and Allende needed time to win support from a majority of the population. Boorstein does take Allende to task, however, for not waging a more aggressive ideological war against his opposition. The president did not make use of revelations that ITT invested heavily to influence the outcome of Chilean elections, and he did not pursue the issue of CIA involvement in Chile's internal affairs. Boorstein believes that Allende could have gained the majority he sought if he had emphasized this outside interference. Another error that Boorstein detects was Allende's failure to call for a plebiscite to establish a unicameral legislature and draft a new constitution after the 1971 municipal elections in which Popular Unity candidates won just over fifty percent of the vote. Allende himself later stated that this was the single greatest mistake he made as president. Despite these criticisms, Boorstein treats Allende sympathetically as a president whose actions could not combat the strong opposition that came from outside his country.

These two books add significantly to the literature on Allende's Chile. Cusack presents a scholarly account enhanced by personal observations made during a four-year stay in Chile as a teacher at Catholic University. Boorstein's book is also strengthened by first-hand impressions which he acquired while working as an economic advisor to the Allende government. While Boorstein's work lacks the objectivity found in Cusack's, it is important because of its balanced treatment of Allende as president and because of four excellent chapters on economic maneuvering carried out by Chile and the United States in the first years of this decade.

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Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

ROBERT E. GALLMAN, editor. *Recent Developments in the Study of Business and Economic History: Essays in Memory of Herman E. Krooss*. (Research in Economic History, Supplement 1, 1977.) Greenwich, Conn.: JAI Press. 1977. Pp. ix, 305. \$27.50.

THOMAS C. COCHRAN, Recollections of Herman Krooss. J. R. T. HUGHES, Krooss on Executive Opinion. RALPH W. HIDEY, Business History: A Bibliographical Essay. STEPHEN M. SALSBUURY, Twentieth Century Railroad Managerial Practices: The Case of the Pennsylvania Railroad. RICHARD SYLLA, Financial Intermediaries in Economic History: Quantitative Research on the Seminal Hypotheses of Lance Davis and Alexander Gerschenkron. MICHAEL D. BORDO and ANNA J. SCHWARTZ, Issues in Monetary Economics and Their Impact on Research in Economic History. RICHARD A. EASTERLIN, Population Issues in American Economic History: A Survey and Critique. PAUL USELDING, Studies of Technology in Economic History. KOZO YAMAMURA, Recent Research in Japanese Economic History, 1600–1945. WOLFRAM FISCHER, Some Recent Developments in the Study of Economic and Business History in Western Germany. RONDO CAMERON, Comparative Economic History.

ERICH KOSTHORST, editor. *Geschichtswissenschaft: Didaktik, Forschung, Theorie*. Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 66. DM 5.80.

KARL-ERNST JEISMANN, Didaktik der Geschichte. Die Wissenschaft von Zustand, Funktion und Veränderung geschichtlicher Vorstellungen im Selbstverständnis der Gegenwart. RUDOLF VIERHAUS, Geschichtsforschung und Didaktik der Geschichte. Didaktische Implikationen der Forschung—Forschungsbezug der Didaktik. JÖRN RÜSEN, Historik und Didaktik. Ort und Funktion der Geschichtstheorie im Zusammenhang von Geschichtsforschung und historischer Bildung.

JOHN T. A. KOUMOULIDES, editor; with the assistance of DOMNA VISVIZI DONTAS. *Greece in Transition: Essays in the History of Modern Greece, 1821–1974*. London: Zeno. 1977. Pp. xii, 334. Cloth £10.00, paper £8.00.

DOUGLAS DAKIN, The Formation of the Greek State: Political Developments until 1923. YANIS YANOULOPOULOS, Greece: Political and Constitutional Developments, 1924–1974. C. M. WOODHOUSE, Diplomatic Development, Nineteenth and Twentieth Century. CHARLES A. FRAZEE, Church and State in Greece. ROBIN FLETCHER, Cultural and Intellectual Developments 1821–1911. S. VICTOR PAPACOSMA, The Military in Greek Politics: A Historical Survey. GEORGE K. ZAHAROPOULOS, The Monarchy and Politics in Modern Greece. JOHN DIMAKIS, The Greek Press. JOHN O. IATRIDES, Greece and the Origins of the Cold War. THOMAS W. DAVIS, JR., The Role of the United States in the Cyprus Dispute—Past and Present. WILLIAM ST. CLAIR, The Philhellenes and the War of Independence. KEITH R. LEGG, The Nature of the Modern Greek State.

P. I. ROSHCHEVSKII, editor. *Klassovaia bor'ba i obshchestvenno-politicheskaia zhizn' dorevoliutsionnoi Rossii* [Class Struggle and the Social-Political Life of Pre-revolutionary Russia]. (Nauchnye trudy, number 8.) Tiumen': Tiumenskii gosudarstvennyi universitet. 1975. Pp. 104. 80 k.

I. V. STEPANOV, K voprosu o dvizhushchikh silakh krest'ianskoi voyny 1670–1671 godov v Rossii [On the Question of the Motivating Forces in the Peasant War of 1670–71 in Russia]. P. I. ROSHCHEVSKII, Politicheskie deiateli XVI–XVIII vekov v tobol'skom izgnanii [Political Figures of the Sixteenth to Eighteenth Centuries in Exile in Tobol'sk]. D. I. KOPYLOV, Iz istorii sibirskikh predkov D. I. Mendeleeva [From the History of the Siberian Ancestors of D. I. Mendeleev]. F. I. BERELEVICH, P. Ia. Chaadaev i A. Sen-Simon [P. Ia. Chaadaev and H. Saint-Simon]. P. I. ROSHCHEVSKII, Razmeshchenie dekabristov v Tobol'skoi gubernii [The Distribution of Decembrists in Tobol'sk Province]. L. P. ROSHCHEVSKAIA, "Surgut'skaia istoriia" (Volneniia politicheskikh ssyl'nykh 80-kh godov XIX veka v Zapadnoi Sibiri) ["Surgut Story" (Riots by Political Exiles in the 1880s in Western Siberia)]. L. E. FAIN, Sotsial'no-ekonomicheskaiia kharakteristika dorevoliutsionnoi kooperatsii

Rossii [The Social-Economic Character of the Pre-revolutionary Cooperative Society in Russia].

CHÜN-TU HSÜEH, editor. *Dimensions of China's Foreign Relations*. (Praeger Special Studies in International Politics and Government.) New York: Praeger Publishers. 1977. Pp. xiv, 293.

DAVIS B. BOBROW, Activity, Purpose, and Performance. STEVEN I. LEVINE, Soviet-American Rivalry in Manchuria and the Cold War. CHÜN-TU HSÜEH and ROBERT C. NORTH, Peking's Perceptions of Soviet-American Relations. KING C. CHEN, China's Response to the United States: From Vietnam to Rapprochement. A. M. HALPERN, China and Japan Since Normalization. CHÜN-TU HSÜEH, Korea in China's Foreign Policy. YITZHAK SCHICOR, The Palestinians and China's Foreign Policy. ROBERT L. WORDEN, China's Foreign Relations with Latin America. SHAO-CHUAN LENG, The PRC's Policy of Protecting Chinese Nationals Abroad. ROGER L. DIAL, Defense of Diplomatic Functions and Ideals During the Cultural Revolution: The Nepal Case.

F. P. KING, editor. *Oceania and Beyond: Essays on the Pacific since 1945*. Westport, Conn.: Greenwood Press. 1977. Pp. xxii, 265. \$22.50.

LAZARUS E. SALII, Liberation and Conquest in Micronesia. SHERWOOD LINGENFELTER and DANIEL T. HUGHES, The Democratization of the United States Trust Territory of the Pacific Islands. DIRK A. BALLENDORF and HOWARD SEAY, Catalysts or Barnacles in Micronesia: The First Five Years of the Peace Corps. MONIKA KEHOE, Language and Politics in Guam and Micronesia. HAMLET J. BARRY III, The Marshall Islands. NOEL GROGAN, Dependency and Nondevelopment in Micronesia. C. HARTLEY GRATTAN, Australia and New Zealand and Pacific-Asia. W. T. ROY, Dilemmas of New Zealand Defense in the Seventies. FELISE VA'A, Western Samoa: A New Zealand Experiment. RALPH R. PREMDAS, Toward a Papua New Guinea Foreign Policy: Constraints and Choice. JOHN KEARSLEY THOMSON, Economic Development in the South Pacific: Some Problems and Prospects. W. DONALD MCTAGGART, New Caledonia and the French Connection. WILLIAM E. TAGUPA, France, French Polynesia, and the South Pacific in the Nuclear Age. RICHARD A. HERR, Tropical Torpor: The United States in the South Pacific Commission. DONALD D. JOHNSON, American Impact on the Pacific Islands since World War II. BARRIE MACDONALD, Imperial Remnants: Decolonization and Political Change in the British Pacific Islands.

L'Amérique des lumières: Partie littéraire du colloque du bicentenaire de l'indépendance américaine (1776-1976). (Histoire des idées et critique littéraire, number 168.) Geneva: Librairie Droz. 1977. Pp. xii, 204.

YVES MORAUD, De La Hontan à Chateaubriand: L'Amérique ou l'exigence utopique de l'unité. PAUL BENHAMOU, Variations du mirage américain au siècle des Lumières. CHRISTIAN AMAT, Un missionnaire-philosophe

face à l'Amérique. PIERRE PEYRONNET, Un personnage américain dans le théâtre français des XVII^e et XVIII^e siècles: Le Sauvage. JEAN SGARD, Prévost et l'espérance américaine. LILIAN WILLENS, Voltaire et l'Amérique: Des colonies anglaises aux treize colonies. JEAN DAGEN, L'Amérique et les Américains dans la philosophie voltairienne de l'histoire. CLAUDINE HUNTING, Paradoxe sur "les Américains" dans l'*Encyclopédie*. JEANINE EON, Jean-Jacques Rousseau et l'Amérique. JACQUES WAGNER, L'Amérique et le *Journal encyclopédique*. LLOYD R. FREE, Images de l'Amérique chez Caron de Beaumarchais. EDOUARD GUITTON, *L'Amérique* de Chénier: une tentative de reconstitution. MERETE GREVLUND, Métamorphoses de l'image de l'Amérique chez Chateaubriand. FRANÇOIS PUPIL, Représentations allégoriques de l'Amérique au moment de la Guerre d'Indépendance. RÉMY LANDY, Après un concert de musique de chambre américaine (1780-1800). JEAN-PIERRE GUICCIARDI, Longchamps, Mably, Raynal et les autres: quelques images de l'Amérique révolutionnaire.

GEORGE W. CORNER, editor. *Aspects of American Liberty: Philosophical, Historical, and Political*. (Memoirs of the American Philosophical Society, 118.) Philadelphia: The Society. 1977. Pp. viii, 233. \$10.00.

ARCHIBALD MACLEISH, Bicentennial of What? SIR RONALD SYME, Liberty in Classical Antiquity. JOSEPH R. STRAYER, The Rule of Law. CHARLES C. GILLISPIE, The Liberating Influence of Science in History. ROBERT R. PALMER, The European Enlightenment in Its American Setting. SYDNEY E. AHLSTROM, Diversity in Religion as a Force for Liberty: A Quadricentennial View of the Problem. CAROLINE ROBBINS, The Efforts of William Penn to Lay a Foundation for Future Ages. JAMES H. HUTSON, Tentative Moves toward Intercolonial Union. EDMUND S. MORGAN, The Problem of Popular Sovereignty. CECILIA M. KENYON, The Declaration of Independence: Philosophy of Government in a Free Society. HARRY W. JONES, The Articles of Confederation and the Creation of a Federal System. GERALD GUNTHER, Toward "A More Perfect Union": Framing and Implementing the Distinctive Nation-building Elements of the Constitution. RENE J. DUBOS, Creative Adaptations to the Future. ROBERT H. BORK, Can Democratic Government Survive? DAVID E. LILIENTHAL, The Rebirth of a Nation. EDGAR P. RICHARDSON, George Washington: The Evidence of the Portraits.

LEROY H. FISCHER, editor. *The Western Territories in the Civil War*. Manhattan, Kan.: Journal of the West. 1977. Pp. 120. \$6.00.

KENNY L. BROWN, Dakota and Montana Territories. SARA JANE RICHTER, Washington and Idaho Territories. JAMES THOMAS, Nevada Territory. GARY L. WATTERS, Utah Territory. THOMAS D. ISERN, Colorado Territory. LARRY D. DUKE, Nebraska Territory. JAMES H. HOWARD II, New Mexico and Arizona Territories. JAMES W. WARE, Indian Territory.

HANS L. TREFOUSSE, editor. *Toward a New View of America: Essays in Honor of Arthur C. Cole*. New York:

Burt Franklin and Company. 1977. Pp. xii, 230. \$18.95.

ARTHUR C. COLE, Southward Ho! First Lecture of the Walter Lynwood Fleming Lectures, Louisiana State University, 1944. ROBERT A. EAST, The Strange Pause in John Adams's Diary. JOSEPH G. RAYBACK, The Presidential Ambitions of Aaron Burr, 1800-1801. EUGENE D. GENOVESE, "Rather Be a Nigger Than a Poor White Man": Slave Perceptions of Southern Yeomen and Poor Whites. MORTON ROTHSTEIN, The Natchez Nabobs: Kinship and Friendship in an Economic Elite. LOUIS FILLER, *The Independent*: Indispensable Conservative in the Antislavery Crusade. HANS L. TREFOUSSE, Andrew Johnson and the Failure of Reconstruction. MILTON CANTOR, United States Foreign Policy and Its Domestic Critics, 1900-1913. MELVYN DUBOFSKY, Tom Mann and William D. Haywood: Culture, Personality, and Comparative History. FRED GREENBAUM, The Foreign Policy of Progressive Irreconcilables.

GERHARD K. FRIESEN and WALTER SCHATZBERG, editors. *The German Contribution to the Building of the Americas: Studies in Honor of Karl J. R. Arndt*. Worcester: Clark University Press; distributed by University Press of New England, Hanover, N.H. 1977. Pp. xvi, 410. \$20.00.

DUNCAN SMITH, "... beschreibung eyner Landschafft der Wilden/Nacketen/Grimmigen Menschfresser Leuthen": The German Image of America in the Sixteenth Century. MARIANNE O. DE BOOP, Deutsche Einwanderung nach Mexico in vier Jahrhunderten. ALEXANDER WALDENRATH, The Pennsylvania-Germans: Development of Their Printing and Their Newspress in the War for American Independence. HAROLD JANTZ, German Men of Letters in the Early United States. LIESELOTTE E. KURTH-VOIGT, The Reception of C. M. Wieland in America. JOHN ANDRE and HARTMUT FROESCHLE, The American Expedition of Emperor Joseph II and Bernhard Moll's Silhouettes. JOHN T. KRUMPELMANN, Charles Sealsfield and Weimar. ALEXANDER RITTER, Sealsfields Erzählformel Seiner Amerika-Romane: Raum und Zeit als Welt und Geschichte. Anmerkungen zur Erzähltheorie am Beispiel des Romans *Kajütenbuch*. ROBERT E. CAZDEN, Johann Georg Wesselhöft and the German Book Trade in America. GERHARD K. FRIESEN, Forgotten Scion of a Famous Family: Carlos von Gagern, Nineteenth-Century Cosmopolitan and Mexican Patriot. ERICH A. ALBRECHT, Eduard Dorsch and the Civil War. ADOLF E. SCHROEDER, The Survival of German Traditions in Missouri. KARL FOUQUET, Die deutsch-brasilianische Schule, ein Werk der

Selbsthilfe. PETER HORWATH, Hermann J. Oberth. The Father of Space Travel: American Perspectives. HEINRICH MEYER, Some Remarks About German-American Relations. ROBERT E. WARD, The Case for German-American Literature. FRITZ SENN, Six Poems.

GEORGE MCCUE, editor. *Music in American Society, 1776-1976: From Puritan Hymn to Synthesizer*. New Brunswick, N.J.: Transaction Books. 1977. Pp. 201. Cloth \$14.95, paper \$3.95.

WILLIAM SCHUMAN, Americanism in Music: A Composer's View. EDWARD JABLONSKI, Music with an American Accent. AUSTIN B. CASWELL, Social and Moral Music: The Hymn. ROY V. MAGERS, Charles Ives's Optimism: or, The Program's Progress. KENNETH B. BILLUPS, The Other Side of Black Music. CHARLOTTE J. FRISBIE, The Music of American Indians. JOSEPH C. HICKERSON, American Folksong: Some Comments on the History of Its Collection and Archiving. K. PETER ETZKORN, Popular Music: The Sounds of the Many. DAN MORGENSTERN, Jazz as an Urban Music. JOHN EATON, The Exhilarating Adventure of New Music in the U.S.A. since 1950. MARY ELAINE WALLACE, American Musical Theater. FRANK PETERS, Musical Corporations in America.

ANN M. PESCATELLO, editor. *Old Roots in New Lands: Historical and Anthropological Perspectives on Black Experiences in the Americas*. (Contributions in Afro-American and African Studies, number 31.) Westport, Conn.: Greenwood Press. 1977. Pp. xviii, 301. \$17.95.

ANN M. PESCATELLO, The Afro-American in Historical Perspective. MARY C. KARASCH, The African Heritage of Rio de Janeiro. ANN M. PESCATELLO, *Prêto* Power, Brazilian Style: Modes of Re-Actions to Slavery in the Nineteenth Century. B. EDWARD PIERCE, The Historical Context of Nengre Kinship and Residence: Ethnohistory of the Family Organization of Lower Status Creoles in Paramaribo. MICHAEL D. OLIEN, The Adaptation of West Indian Blacks to North American and Hispanic Culture in Costa Rica. MARY W. HELMS, Negro or Indian?: The Changing Identity of a Frontier Population. ROGER ABRAHAMS, The West Indian Tea Meeting: An Essay in Civilization. DOUGLAS MIDGETT, West Indian Version: Literature, History, and Identity. NORMAN E. WHITTEN, JR., Ritual Enactment of Sex Roles in the Pacific Lowlands of Ecuador-Colombia. MICHAEL CRATON, Perceptions of Slavery: A Preliminary Excursion into the Possibilities of Oral History in Rural Jamaica.

Other Books Received

Books listed were received by the *AHR* between October 1 and December 1, 1977. Books that will be reviewed are not generally listed, but listing does not necessarily preclude subsequent review.

GENERAL

- BARZEL, YORAM, and HALL, CHRISTOPHER D. *The Political Economy of the Oil Import Quota*. (Hoover Institution Publication, number 172.) Stanford, Calif.: Hoover Institution Press. 1977. Pp. 96. \$8.95.
- BAUM, WILLA K. *Transcribing and Editing Oral History*. Nashville, Tenn.: American Association for State and Local History. 1977. Pp. 127. \$6.75.
- BUNZL, JOHN. *Klassenkampf in der Diaspora: Zur Geschichte der jüdischen Arbeiterbewegung*. Foreword by KARL R. STADLER. (Schriftenreihe des Ludwig-Boltzmann-Instituts für Geschichte der Arbeiterbewegung, number 5.) Vicana: Europaverlag. 1975. Pp. 183.
- CARNER, MOSCO. *Alban Berg: The Man and the Work*. New York: Holmes and Meier. 1975. Pp. xv, 255. \$24.00.
- CHAMBERLAIN, JOHN. *The Roots of Capitalism*. Indianapolis: Liberty Press. 1976. Pp. 293. Cloth \$9.00, paper \$3.00.
- CIPOLLA, CARLO M. *Before the Industrial Revolution: European Society and Economy, 1000-1700*. New York: W. W. Norton and Company. 1976. Pp. xiv, 326. Cloth \$12.95, paper \$4.95.
- DE QUEIRÓS MATTOSO, KÁTIA M., editor. *Textos e documentos para o estudo da história contemporânea (1789-1963)*. (Coleção Textos, number 3.) São Paulo: HUCITEC. 1977. Pp. xxv, 209.
- ENGEL-JANOSI, FRIEDRICH, et al., editors. *Formen der europäischen Aufklärung: Untersuchungen zur Situation von Christentum, Bildung und Wissenschaft im 18. Jahrhundert*. (Wiener Beiträge zur Geschichte der Neuzeit, number 3.) Munich: R. Oldenbourg Verlag. 1976. Pp. 274. DM 54.
- FELD, MAURY D. *The Structure of Violence: Armed Forces as Social Systems*. Preface by CHARLES C. MOSKOS, JR. (Sage Series on Armed Forces and Society, number 10.) Beverly Hills, Calif.: Sage Publications. 1977. Pp. 203. \$12.50.
- FROLOVA, I. I. *Inostrannyye spravochniki po istorii zarubezhnykh stran: Evropa, Amerika, Australiia; Annotirovannyi ukazatel' literatury* [Foreign Reference Works on the History of Foreign Lands: Europe, America, Australia; An Annotated Index of the Literature]. Moscow: Izdatel'stvo "Kniga." 1976. Pp. 143. 46 k.
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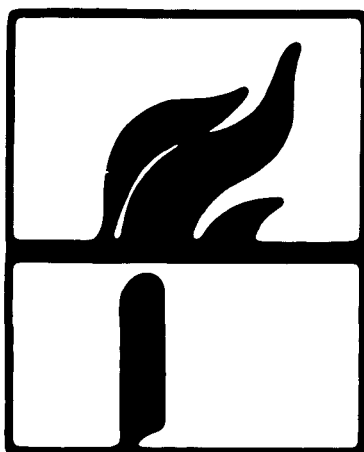
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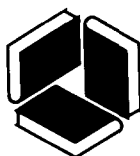
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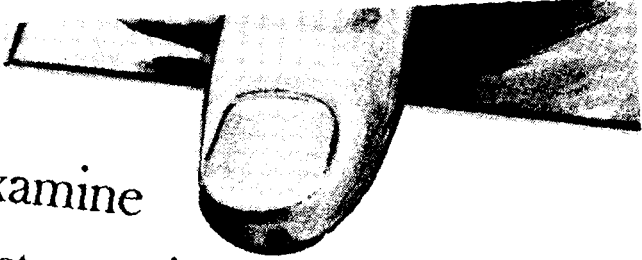
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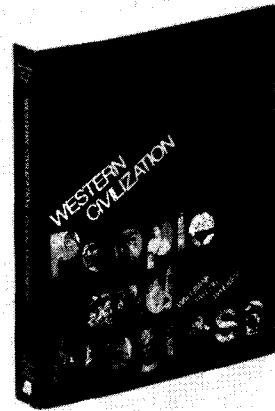
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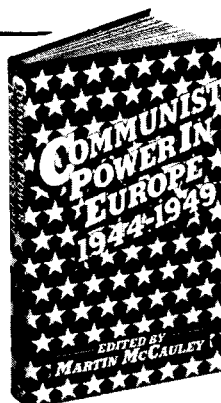
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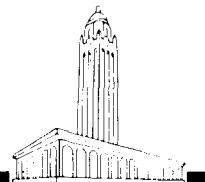
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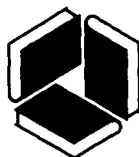
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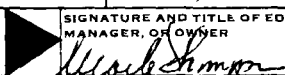

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